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EDWARD WARREN, M.D.

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A REVIEW OF DR. OTIS FREDERICK MANSON'S ESSAY ON "MALARIAL PNEUMONIA."

BY W. T. HOWARD, M. D., OF WARRENTON, N. C.

[*Presented to the State Medical Society of North Carolina May*, 1858.]

(CONTINUED.)

IX.—In continuation of his argument against the *crepitant ronchus*, Dr. Manson alleges:

"Again, it often occurs, in the early stages of this affection, that the cough is unattended with expectoration for several days; indeed, we have seen cases of the most violent character, terminating in death by apnœa, with expectoration scarcely sufficient in quantity to betray the character of the affection, the cough being dry throughout, and the dyspnœa extreme. Now, in these cases, we should be prepared to learn that no crepitation would be heard, or, if observed, not in proportion to the extent or gravity of the pulmonary lesion."

1. If the cough was "*dry*," and the expectoration, of course, wanting, and there were no physical signs to "be heard," how could Dr. Manson know that the disease was *pneumonia?* Is a dry, unproductive cough, with great difficulty of breathing, sufficient to make the diagnosis of this disease? Surely not. Then the subsequent progress of the case must determine the diagnosis. In these instances, in which there are no diagnostic signs or symptoms of pneumonia, and which are of *rare* occurrence, the crepitant ronchus is certainly not less available than other indications of pneumonia in its first stage, the value and importance of which Dr. Manson duly estimates, and nowhere depreciates. Why, then, particularly disparage the *crepitant*

ronchus? The cases referred to by Dr. Manson are evidently instances of *central pneumonia*, in which there is certainly nothing peculiar to the "malarial" form. Indeed, it is only another of the many points of resemblance between Dr. Manson's "malarial pneumonia" and ordinary simple pneumonia, accidentally impressed by a malarious influence. "It is now well ascertained," says Fournet, "contrary to the opinion of Lænnec, that, in central pneumonia, the most careful auscultation cannot recognize any of the usual local signs of the disease. During the year 1837, there were three patients under the care of M. Andral, at *la Charité*, who were affected with central pneumonia, characterized by the sputa and the general symptoms, in all of whom the above was true. Some patients, in whom I have traced, with the utmost care, and, as I may say, from hour to hour, the progress of a pneumonia that I had seen arise under my own eyes, have had no primary crepitating rhonchus at the commencement of the disease, and no rhonchus redux at the period of resolution. Perhaps this usually occurs when the primary rhonchus is wanting." (*a*). Other authors, also, have long ago called attention to these cases, particularly Andral (*b*), Chomel (*c*), Williams (*d*), &c. Indeed, it would be impossible, we presume, at the present time, to find any respectable authorities maintaining different grounds; and yet, as it seems to us, Dr. Manson speaks of such instances as if they were encountered only in the "malarial" form of pneumonia! Subsequently, however, in alluding to them, he says: "In these cases the pneumonia is either central, or the secretion in the air-cells and finer branches of the bronchi may not be present in quantity sufficient to offer any obstacle to the transmission of air." Still, by stating in the very next sentence—"Whatever explanation, however, may be offered, I am fully persuaded, after careful exploration of the chest in many cases daily and hourly made, that *at least*, in the early stage of *this form of pneumonia*, no abnormal sounds can often be observed,"—the reader is led to infer by obvious, if not necessary implication, that in all this

(*a*) Op. cit., 121.
(*b*) Med. Clinic, vol. ii., 189.
(*c*) Dict. de Med., tome xxv., 185.
(*d*) Cyc. Prac. Med., vol. iii., 607.

there is something rather peculiar to, if not positively distinctive of, Dr. Manson's "malarial pneumonia." It can scarcely be necessary to add, that such an assumption on the part of Dr. Manson is as ill-founded as it is gratuitous. If, "after careful explorations of the chest, daily and hourly made," in the early stage of certain cases of "*malarial* pneumonia," Dr. Manson could not hear the crepitant rhonchus, nor any "abnormal sounds"; so, also, M. Jules Fournet, with all his great acuteness, could not recognize the crepitant rhonchus, nor "any of the usual local signs of the disease," in some patients affected with *simple central pneumonia*, that had arisen under his own eyes, although he traced the progress of the disease "from hour to hour," and "with the utmost care." Verily, one might reasonably conclude, from the remarkable coincidence of their language, that these writers observed the *same* "form of *pneumonic lesion*"; and such, doubtless, was the fact; for why should not "*malarial*," as well as other varieties of pneumonia, sometimes occupy central portions of the pulmonary parenchyma?

Considering it, then, as well established, that there are some cases of central pneumonia, whether they be characterized by the evidences of a malarious implication or not, in which, especially at the onset, no physical signs of the disease can be recognized, it is yet equally true that, as the inflammation approaches the surface, we may discover the appropriate auscultatory phenomena in the immense majority of instances, unless the amount of lung substance invaded be small, and circumscribed on all sides by a large mass of normal pulmonary tissue. We do not doubt, however, that cases of central pneumonia are sometimes met with, which offer no physical signs of their existence during the time that they are under care, and, being successfully treated, are dismissed under the impression that no evidences of disease were to be recognized, by the method of physical exploration, during the entire course of their clinical career; and yet, nevertheless, had those cases remained longer under observation, auscultation and percussion would finally have revealed and localized the affection. In the Fall of 1853 we treated a case which clearly established the truth of this statement. We were

called to a robust negro, ætat. 24, on the third day of his attack. He had been seized, in perfect health, with a severe chill, which was speedily succeeded by dyspnœa, cough, and high fever. At our first visit, the respiration was 48, pulse 126, full and resisting, and there was well-marked tenaceous, rusty-colored expectoration. The most careful physical exploration could discover no sign of disease whatever. The patient was freely bled, and treated mainly with tartar emetic, aided by several moderate doses of calomel during the sickness, administered at bedtime. The case yielded kindly to treatment in eight days; the respiration resumed its normal standard; the pulse fell to 60 in the minute; the tongue presented a clean and healthy aspect; the appetite was keen; and, in a word, the patient seemed to be "*well.*" Having other cases at the same house that required our attendance for several weeks, we determined to auscult the chest of the pneumonic convalescent every day, with great care. About the tenth day from the date of seizure, and while he was walking about his cabin, anxious to go out of doors and resume full diet, we detected distant, but unmistakable bronchial respiration, and weak bronchophony, about an inch and a half below the spine of the left scapula. In twenty-four hours there was well-marked dullness on percussion, and loud bronchial respiration and bronchophony. As the patient appeared, in every other respect, to be perfectly restored to health (save only the natural loss of strength induced by his attack), no remedial agents were administered; the case was closely watched, and the lung cleared up beautifully in four or five days. Now, had this patient passed from under observation at the time that convalescence seemed so well established—as he would have done, but for our accidental further attendance at the same house upon other cases,—we should ever afterwards have thought, and with apparent good reason, that a central pneumonia had existed, which never presented any of the ordinary physical signs of the disease. We suspect that others have encountered similar cases, which were not recognized, simply because the patients passed from under care so soon as convalescence seemed to be firm and unshackled.

We will remark, *en passant*, that we have several times seen patients walking about in their chambers, with every vital indication of unfettered recovery, no inconsiderable portion of one of whose lungs presented, notwithstanding, the well-marked physical signs of hepatization. "I have had under my charge a girl four years of age," says Dr. J. Forsyth Meigs, in his incomparable Treatise on the Diseases of Children, "who, on the seventh day of the attack, after being leeched on the fifth, had a respiration of 20, a pulse of 90, and a cool natural skin, who was, in fact, entirely convalescent, in whom, nevertheless, there was dullness on percussion over the lower half of the right lung, with bronchophony and bronchial respiration." (*a*)

2. But Dr. Manson says further that, in some cases of "*malarial* pneumonia," the cough, in the early stages, is unattended by expectoration for several days. It is well known, however, that the expectoration is not observed in some cases of ordinary simple pneumonia for several days after the onset; and there are some cases, also, in which it never occurs at all. (*b*) Thus, Grisolle has seen fourteen cases that never had expectoration at any stage of the malady. (*c*) According to Bouillaud, it is oftener wanting in pneumonia of the summit than when the disease occupies the lower lobes. In *five* of Grisolle's cases the inflammation affected the *base;* in *two*, the *middle;* and in *seven*, the *summit*. The following table given by Grisolle, in which the characteristic sputum was observed for the first time in 131 cases of simple pneumonia, shows beyond all question that its non-appearance, in the early stages of certain cases of the "malarial" form, for several days, is not by any means peculiar to that variety of the disease; and, at the same time, offers another point of coincidence between Dr. Manson's "malarial pneumonia" and ordinary simple pneumonia, presenting the accidental tokens of the periodic fever impress. The peculiar expectoration appeared—

(*a*) Op. cit., second edition, 167.

(*b*) C. J. B. Williams, Cyc. Prac. Med., vol. iii., 616; Andral, Med. Clin., vol. ii., 190; Stokes, Dis. of the Chest, 274; Swett, Dis. of the Chest, 98; Graves, Clinical Lectures, 2d Amer. edit., 59; Wood, Prac. Med., vol. ii., 11, &c.

(*c*) Traité Pratique de la Pneumonie, 221.

On the	first	day, in	45	cases.	On the	sixth	day, in 6 cases.
"	second	"	31	"	"	seventh	" 5 "
"	third	"	14	"	"	eighth	" 2 "
"	fourth	"	14	"	"	eleventh	" 2 "
"	fifth	"	11	"	"	twelfth	" 1 case. (*a*)

Total, 131 cases.

3. There are yet some most violent cases of "*malarial* pneumonia," says Dr. Manson, in which the expectoration is "scarcely sufficient in quantity to betray the character of the affection." But if it was "*sufficient in quantity to betray the character of the affection*," then it was abundant enough for the production of the crepitant rhonchus, according to either of the theories which Dr. Manson has brought forward in explanation of the mechanism of this rhonchus; and the extent of surface over which the rhonchus might be heard would indicate the amount of pulmonary parenchyma invaded, unless the pneumonia were *central*, or at least so far removed from the surface as to prevent the recognition of the râle. Moreover, does not every medical man know that the exudation of which the sputum is composed is not unfrequently retained—not *expectorated*—when it is in the air-cells; and hence, the crepitant rhonchus may exist, although the *expectoration* be wanting? It so happens that, since we commenced writing this Review, we have seen two cases of what Dr. Manson terms "malarial pneumonia," in both of which this point was illustrated in the clearest manner. We saw in a previous page that Grisolle has only met with *four* cases of pneumonia in his large experience, in which the crepitant rhonchus was not observed; and yet he tells of *fourteen* cases in which the expectoration was wanting throughout their career.

In regard to the *quantity* of the matters expectorated, every physician must have observed that in simple pneumonia it is very variable, and in some cases that there is scarcely any expectoration at all. Gerhard states that it is small in amount, generally from one to four ounces in twenty-four hours, and that its becoming more abundant is usually a sign that the disease is retrograding. (*b*)

(*a*) Op. cit., 219.

(*b*) Op. cit., 143.

X. The next step taken by Dr. Manson is, to sap the very foundation of the crepitant rhonchus. He urges that—

"In describing the anatomical characters of pneumonia, Lænnec admits that inflammation of the bronchia is a frequent attendant on the simple form, 'its lining membrane being commonly very red,' 'occasionally swollen,' 'the redness sometimes extending over the whole bronchi.' Now, if this be true, and who will gainsay it, how is it possible that sonorous, sibilous and mucous rhonchi should not be often heard, as maintained by Skoda, and if present, drowning, according to Andral, the less audible sound of crepitation."

1. By referring to the context—(see the June number of this journal),—the reader will perceive that the above extract from Dr. Manson's description of the "physical signs, &c.," of "malarial phenomena," comes in just after his attempt to account for the alleged absence of the crepitant rhonchus in the large majority of cases, upon the postulate, that, in this variety of pneumonia, "the pathological condition of the lung" is "widely different from that described" by *Lænnec* and the "majority of authors." Abandoning the previous line of his argument, Doctor Manson makes bold to deny that the crepitant rhonchus can be often recognized even in cases of simple sthenic pneumonia, on the hypothesis that, from Lænnec's own description of its anatomical characters, sibilous and mucous rhonchi should not be *often* heard, as maintained by Skoda, and, if present, *drowning*, according to Andral, the *less audible sound of crepitation.*" Now, even if all this were true in regard to *simple sthenic pneumonia*, it cannot apply to the "malarial" form, unless Dr. Manson is prepared to admit what he so strenuously denies, viz., that their pathological anatomy, at least in so far as his citations from Lænnec are concerned, is identical. If, therefore, according to his own reasoning, all this is intended to have any application to his "malarial pneumonia," Dr. Manson must either give up his views respecting its true pathological anatomy—its character and nature,—or he must drop his objections.

2. But let us examine the validity of Dr. Manson's logic in the premises. Skoda is first cited, to prove that bronchial râles are "*often* heard" in pneumonia, and Andral to show that, if present, "the less audible sound of crepitation is *drowned*": so

that, between the two, the crepitant rhonchus fares badly—is completely stifled indeed!

Is this mode of reasoning correct? Is it consistent with the received rules of sound logic? An isolated idea is cited from one author; another isolated idea is cited from another author; and from these premises, thus torn from their context, is drawn a conclusion at variance with the obvious meaning of each, and from which both would doubtless revolt!

That *bronchitis* (accompanied by its appropriate physical signs) *may* be associated with pneumonia, there can be no doubt; but that this complication is *clinically* so constant, and exists to such an extent as to prevent the recognition of the *crepitant rhonchus*, there is not, so far as we are informed, any proof whatever. What is the testimony of *Skoda* on this head? He simply states (in the page referred to by Dr. Manson), that infiltration of lung-tissue, associated with fluid secretions in the air passages, produces the signs of bronchial catarrh, and that the nature of the râles heard in pneumonia depends upon the situation of the secretion, whether it be present in the finer bronchial tubes and air-cells alone, or in these and the larger bronchial tubes also, or in the larger bronchial tubes only, &c.; and, in a subsequent page (as we have already seen), he affirms that he meets with the unequal bubbling (*sub-crepitant?*) râle, or whistling and sonorous sounds (*i.e.*, sonorous and sibilant ronchi), much more often at the commencement of pneumonia, than the fine equal-bubbling (true *crepitant*) rale (*a*); but, certainly, not a word is said about bronchial râles "*drowning*" the *crepitant rhonchus.*

3. What, then, has *Andral* to say about the matter? The reader doubtless remembers the lucid account (already presented in a previous page), which this author gives of the crepitant ronchus, as heralding pneumonia, *from the very onset, in the majority of cases.* Did we see anything in regard to this rhonchus being "*drowned*" by bronchial râles? Dr. Manson, however, refers to a note by Andral in Lænnec's treatise, as authority for the statement he makes. Let us examine it. But

(*a*) Op. cit., 306, 321, '2.

we must first see what was the language used by Lænnec, upon which Andral made the note in question. Lænnec states that—

"The sero-sanguineous congestion of the lungs, which takes place in almost all dying persons, is frequently converted into pneumonia, if the agony is at all protracted"; and he calls this "*pneumonia of the dying*," which "is commonly accompanied with a *very strong and suffocating tracheal rhonchus.* * * *This* rhonchus, when extremely strong, is unquestionably the thing most apt to mask the crepitous rhonchus of incipient pneumonia. Andral encountered this difficulty, and would seem to consider it as insurmountable." (*a*)

In commenting upon this language, Andral adds the following note, referred to by Dr. Manson:

"Notwithstanding the opinion of Lænnec, I still believe that in cases where there is *a noisy mucous rhonchus, like the one here described*, auscultation cannot possibly reveal the existence of pneumonia, unless the disease be sufficiently violent to give rise to the bronchial murmur: this is often heard through the bubbles of the mucous rhonchus, especially when the patient is made to breathe hard." (*b*)

And this is the evidence upon which Dr. Manson bases the broad statement that, according to Andral, the *sonorous, sibilous and mucous ronchi* of bronchitis, when present in pneumonia, "*drown the less audible sound of crepitation!*" Andral simply affirms that "a noisy mucous rhonchus, like" "a very strong and suffocating tracheal rhonchus," prevents the recognition of pneumonia, unless it be advanced to the stage of hepatization; and even this statement may be fairly referred to the pneumonia of the dying, since it was of this that *Lænnec* was speaking, and upon which Andral made the note we have quoted. Dr. Manson had just cited on the opposite page Andral's Medical Clinic—thus, "Cl. Med., tome ii., 333"—for his opinions of the crepitant rhonchus; and from it Dr. Manson saw this author's views of this râle presented *in extenso*. Why, then, seek them in a short note relating to pneumonia developed in the last moments of life—if, forsooth, it can justly be called pneumonia at all,—when its recognition, even could it be made, would not be of the least practical importance? Indeed, we confess ourselves wholly at a loss to conceive how Dr. Manson could have so completely misapprehended Andral's obvious meaning. In a previous note

(*a*) Op. cit., Amer. edit. (1833), 247. (*b*) Lænnec's Work, loc. cit.

to Lænnec's Treatise (*a*), he saw that Andral represents the crepitant rhonchus as "occurring *almost constantly* in the first stage of pneumonia." Now, if in pneumonia this râle were so "often" "*drowned*" by bronchial râles as *Dr. Manson* supposes, how could Andral have observed it "ALMOST CONSTANTLY"? It is very true that Andral states, in his Medical Clinic, that "it sometimes happens that the great quantity of liquid accumulated in the bronchi occasions a râle so loud as to mask all other sounds, so that the state of the pulmonary parenchyma can no longer be known by auscultation" (*b*); but how often does this happen in the first stage of pneumonia? Not often, certainly, in *Andral's* experience, one would say.

In short, it is perfectly evident that, in the note to which Dr. Harrison refers, Andral had no allusion whatever to the immense majority of cases of pneumonia, as they are ordinarily encountered at the bedside; that Dr. Manson has strangely mistaken his plain meaning, and hence, that Dr. Manson's deductions, in the premises, are illegitimate and without weight.

But why should Dr. Manson *theorize* about a matter of fact? *What does observation teach?* In his work on the Diseases of India, Dr. Morehead reports three cases of *primary pneumonia* in which the crepitant rhonchus was observed, although bronchial râles were present likewise; and five cases of *febrile pneumonia*,—by which, for the sake of convenience, he designates the pneumonia which complicates intermittent or remittent fever,—in *four* of which the crepitant ronchus was heard, notwithstanding the association of bronchial râles. Of 240 cases of pneumonia observed by Grisolle, only 58 had distinct stethoscopic signs of *bronchitis;* and we have already seen that he never met with but *four* cases of pneumonia in his whole life, in which the crepitant ronchus did not occur. Flint states that "the *infrequency* of the occurrence of the bronchial râles, irrespective of the sub-crepitant, in ordinary cases of pneumonitis, is shown by the following: of 148 examinations at different periods in 45 cases, a sibilant is noticed in seven, a sonorous in six, and a mucous in three instances." The sub-crepitant râle,

(*a*) Op. cit., 222. (*b*) Op. cit., vol. ii., 189.

he says, "*may* occur at any period of the disease;" but "exclusive of the *very rare instances* in which pneumonitis and capillary bronchitis are associated, the sub-crepitant râle is much more likely to occur at a late period in the disease, during the progress of resolution. Developed under these circumstances it is, in fact, the *returning crepitant râle of Lænnec.*" This writer is careful to insist that these râles (*crepitant* and *sub-crepitant*) may be combined and yet distinguished—"the *crepitant* appearing at the *end* of the inspiration, and the *sub-crepitant* in *both* acts." In proof of this statement, he mentions a case in which capillary bronchitis and pneumonitis were associated—"the fact of the occurrence of the two diseases having been demonstrated after death; the *sub-crepitant* râle existed on *both* sides, but on *one* side the sub-crepitant and crepitant râles were distinctly appreciable during the same inspiration, the former during the first part, and the latter at the close of the act." (*a*) In regard to the *sonorous* rhonchus, Walshe states that it is "singularly uncommon" in the pneumonia of adults (*b*). We are speaking of the disease all along as it appears among adults, and so does Dr. Manson. It is well known that *in children* the co-existence of bronchitis and pneumonia is much more common.

Now, as Dr. Manson seems to think it not "*possible* that sonorous, sibilous, and mucuos rhonchi should not be *often* heard" in pneumonia, "and, if present, *drown* the less audible sound of *crepitation*," we would ask how he accounts for the *fact* that they are *not* "*often heard*" in this disease; and even when they are observed, that they do not often "*drown*" the crepitant rhonchus? And, furthermore, how will he explain the well known, undenied, and undeniable *fact*, that the immense majority of competent judges all over the world describe the crepitant rhonchus as one of the most constant and important signs of pneumonia, if, according to *him*, bronchial râles must necessarily "*often*" occur, and preclude its recognition?

XI. Lastly, in his continuous effort to discredit the value of the crepitant rhonchus, Dr. Manson states, in a note, that—

(*a*) Op. cit., 233, 412, '12. (*b*) Op. cit., 415.

"Lænnec himself only cites two cases of this (malarial) form of pneumonia, in which the glutinous sputa and crepitous rhonchus were observed."

It is evident that, by this passage, Dr. Manson intends to convey the idea that, as Lænnec himself only refers to "*two cases*" of (what Dr. Manson terms) "*malarial pneumonia*," in which the crepitant rhonchus was observed, there were other cases of this character seen by Lænnec, in which this ronchus could not be detected. If this be not the proper construction of Dr. Manson's language, then the statement is entirely destitute of all force; indeed, it has no meaning at all.

Now, it seems to us that, in this matter, apparently without seeing it, Dr. Manson has answered himself. Our author forgets just here, that he had informed his readers in a previous page of a "*fruitless search*" he had made into "the works of the great master of physical diagnosis," while he was endeavoring "to learn the distinctive points of diseases having the same name and location, yet requiring such different modes of cure" (referring to cinchona bark and tartar emetic); but that the "important and lamentable omission is, however, *easily explained by the fact*, that he (Lænnec) witnessed the first mentioned epidemic* *before his attention had been especially directed to the diseases of the chest.*" Why, certainly, Lænnec could not describe a sound he had never heard, and of which he (and the "rest of mankind") had not, at that time, the slightest knowledge; and yet his omission to describe it furnishes Dr. Manson with an *inferential* argument that the sound was not present in certain cases witnessed by Lænnec!

According to Dr. Manson's own admission, however, Lænnec's experience in the class of cases in question, after his attention

* We presume that Dr. Manson has reference to the following passages in Lænnec's Treatise:

"The pneumonic fever may be accompanied by a bilious affection, a complication which was very common towards the close of the last century, but *which is now extremely rare.* Almost all the pneumonias observed by Stoll were bilious; and I was myself witness of many similar cases when I attended the lectures of Crovisort. *Since* 1804, however, I have met with no well-marked example of the kind."

Lænnec's attention was first directed to Auscultation *in* 1816, while he was physician in chief to the Hospital Necker, in Paris.

was specially given to the subject of physical diagnosis, was very limited. But as he certainly did see at least "*two cases*" of the so-called "malarial pneumonia," after he commenced the study of Auscultation, let us look into them, and endeavor to ascertain whether Dr. Manson might not have made a much more logical deduction in the premises. If it should appear that, after Lænnec first directed his attention to the physical exploration of diseases of the chest, these "*two cases*," in which the crepitant rhonchus was present, were the only ones of the kind that he saw; if there be no evidence that he met with other similar cases in which this râle could not be detected; then, Dr. Manson's inference, having no foundation in fact upon which to stand, must fall to the ground; indeed, the tables will be completely turned against him.

In his Treatise on the Diseases of the Chest, Lænnec devotes a section to "latent and symptomatic pneumonia." He notices it as an inter-current affection in various diseases, and concludes the section thus:

"We ought to range among sympathetic pneumonies that which constitutes the predominant symptom in the *pernicious fevers* denominated pneumonic. The morbid anatomy of this affection is very imperfectly known, from the circumstance of its proving rarely fatal; as we fortunately possess in cinchona, when administered in time, a certain means of cure. We have some facts, however, which prove that traces of pneumonia have been found in subjects dead of this disease; I have myself, in two accessions of this fever, witnessed the presence of the glutinous sputa, and a very intense crepitous rhonchus" (*a*)

Now, this is all that Lænnec says on the point under notice; and what is the obvious and only rational inference to be drawn from his own words? Is it not just the very opposite of that which *Dr. Manson* would seek to establish? Lænnec tells us that the morbid anatomy of the *pernicious fevers* called pneumonic is very imperfectly known—this fever being rarely fatal on account of the great virtues of cinchona when timely administered; but that traces of *pneumonia*—of *pulmonary inflammation*—have been found in subjects dead of the fever; and, as additional evidence of that fact, he states: "*I have myself*, in

(*a*) Op. cit., 218, '9.

two accessions of this fever, witnessed the presence of the glutinous sputa, and a *very intense crepitous rhonchus.*" There is not a particle of evidence to show that he ever saw other similar cases in which the crepitant rhonchus was not present; and any mere *inference* that there were other cases of this kind witnessed by him, in which this rhonchus was wanting, is not only a complete *non sequiter*, but is contradicted by his own statement of the "*constant presence*" of the râle in the first stage of pneumonia. He nowhere makes any exception to this law, save in a single instance of slight central pneumonia, "in which *all* the stethoscopic signs were wanting;" and there is not the slightest intimation that this case was in any manner connected with the poison of malaria.

We conclude, therefore, that Dr. Manson's reference to these famous "*two cases*" is most singularly unfortunate for him; for they appear to have been the only cases of that character seen by Lænnec, after his discovery of the immense advantages of auscultation to the science of Diagnosis; and *both* of them were characterized by the presence of a "*very intense crepitous rhonchus.*" Grisolle alludes to these "*two cases*" as the only ones that Lænnec saw, in which pneumonia constituted the predominant symptom in pernicious fever. (*a*)

Let us now inquire what is the testimony of other observers in regard to the occurrence of the crepitant rhonchus, when pneumonia is impressed with a malarial or periodic fever element. We have already seen, that Prof. Wood (see citation from this author in the April number of this Journal, p. 428) affirms that the physical signs do not "materially differ, in this form of pneumonia," from those that are "characteristic of the uncomplicated disease." Drake makes no special allusion to the physical signs in his section on "*Bilious or Malarial Pneumonitis*," doubtless because he considered them as identical with those he had described, in the preceding chapter, as indicative of ordinary simple pneumonia; but, in treating of "Inflammatory Intermittents," in another place, he states: "A sudden change of weather may develop pulmonary inflammation in connection with intermit-

(*a*) Op. cit., 426.

tent fever; this will be indicated by cough, dyspnœa, pain, and the *ordinary auscultatic signs*" (*a*). Swett alludes to the complication of pneumonia with periodic fever (*b*), but says nothing of any modification of the physical signs. Flint—then Professor of Practice in the University of Louisville, now Professor of Clinical Medicine at the New Orleans Medical School—states that, in miasmatic districts, pneumonia is often combined with the phenomena of the periodical fevers, but is silent in regard to any special deviation of the usual physical signs (*c*). Grisolle speaks of the presence of the crepitant râle in malarious cases, and regrets that some contemporary authors have published cases which they have presented as examples of this kind, without even dreaming of giving to their diagnosis the necessary certainty by the aid of the methods of exploration which science teaches at the present time (*d*). In treating of the physical signs of a variety of pneumonia, which, according to him, is caused by "*malaria*, and the agents which promote its production—atmospheric deviations and vegetable decomposition,"—Dr. Ramsay, of Georgia, remarks: "By applying the ear to the chest in the first stage of pneumonia, we will find the natural sound has been displaced by the *crepitating rhonchus.* * * * It is heard best during inspiration, and is *characteristic of pneumonia.* As the disease advances towards the second stage, the crepitant rhonchus ceases, and is followed by bronchial respiration" (*e*). The Editors at that time of the Charleston Medical Journal, Drs. Cain and Porcher (who, according to Dr. Manson, must have had chiefly in view "malarial pneumonia," since, according to him, this "is the form of pneumonia *generally seen by southern observers*)," in commenting on Skoda's views respecting the crepitant râle in pneumonia, hold the following language: "We cannot, of course, judge exactly what Skoda means by following Lænnec's description very closely, but *we do know* that many, very many observers, equally as reliable and trustworthy as he, have recognized the crepitation of pneumonia by Lænnec's description of

(*a*) Op. cit., 67. (*b*) Op. cit., 104, 113.
(*c*) Op. cit., 402. (*d*) Op. cit., 427.
(*e*) Charleston Med. Jour., vol. vi. 8, Dr. Ramsay says he has seen about 370 cases of this form of pneumonia.

it, and have found it an *invaluable and almost constant sign in the diagnosis of the disease.*" (*a*) Dr. Constant, who practises in one of the marshy districts of the department of the Lot (in France), invites attention to the signs which distinguished (what he calls) *intermittent pneumonia.* After speaking of the vital symptoms, he proceeds to state that—"Auscultation and percussion are of the highest value, often revealing the disease when unsuspected;" and "a distinctive feature is the rapid passage from the first to the second stage of the disease, so that eight or twelve hours after auscultation had revealed only a slight *circumscribed râle*, a whole side will be found hepatized. * * The *crepitant râle* of the first stage is almost always moist, the parchment-cracking râle only having been heard for a short period, two or three times in more than sixty cases" (*b*). The reader will remember that, in his descriptions of the crepitant râle, Lænnec states, in one place, it has "a well-marked character of *fluidity*," and in another, that it "seems *hardly* to possess the character of humidity." Constant says that, in his cases of intermittent pneumonia, the crepitant râle was almost always moist; still it was the *crepitant râle* that he heard, and it "*circumscribed*" the disease in its first stage. Finally, in his recent able and elaborate work on the Diseases of India, Dr. Morehead gives the clinical history of five cases of pneumonia complicating the phenomena of periodic fever (to which we have already referred in another connection), in every one of which the crepitant rhonchus was present; and nowhere, in his long article of seventy pages on pneumonia, does he give any intimation that this râle is not heard in that variety of the disease. On the contrary, the physical signs of the affection are everywhere spoken of as identical with those that characterize the ordinary frank disease.

XII. Before concluding the subject in hand, let us briefly glance at the diseases, other than *pneumonia*, in which the crepitant rhonchus is said to occur, and endeavor to determine, with some degree of precision, its real clinical value.

1. *Œdema of the Lungs.* In its acute form, this is, as

(*a*) Op. cit., vol. xi. 786. (*b*) Ranking's Abstract, No. xviii. 74.

Fournet justly remarks (*a*), a very rare disease. Occurring as a chronic and secondary process in the course of other diseases, —as in albuminuria and general dropsy,—it is more common; but whether witnessed in its acute or chronic form, it is excessively rare that a true crepitant rhonchus is observed. As a *rule*, we hear the *sub-crepitant*, a true bubbling-râle, in which the bubbles are larger, moist, less uniform in size, succeeding each other in a continuous train with less abruptness in their evolution, persisting a long time unchanged without being supplanted by the signs of pneumonic condensation, existing without much fever, accompanied by the phenomena of general dropsy, occurring on both sides of the chest, and usually in the postero-inferior portions of the lungs.

2. *Pulmonary apoplexy.* This, also, is a very rare disease, and is very seldom seen as a *primary* affection, if, indeed, it ever occurs, as such, at all. Though for nearly two winters in attendance upon the practice of the Pennsylvania Hospital, the clinics at Jefferson Medical College, for twelve months a resident physician in the large Hospital of Baltimore City and County Alms House, and being for fifteen years actively engaged in private practice, we are not certain that we ever saw a case, and have only suspected its existence in a single instance. Swett—whose experience in private, as well as in the large hospitals in New York and elsewhere, was ample—affirms: "Pulmonary apoplexy is, in my experience, a rare form of disease. During a residence of a year and a half in Paris, where I was constantly in attendance upon the hospitals, especially those in which chest diseases were most frequently to be found, I did not meet with a single case of the disease. Since that time, I have met with a *few* cases, and, I believe, always in connection with one uniform condition of the heart—viz.: disease of the mitral valve, with hypertrophy of the right ventricle" (*b*). He subsequently adds (loc. cit.): "For my own part, I have never discovered *any physical signs* of pulmonary apoplexy." We have seen, in a previous page, that *Walshe* has never met with true crepitation in pulmonary apoplexy, or in the common hæmoptysis of phthi-

(*a*) Op. cit., 162. (*b*) Dis. of the Chest, 185, 6.

sis ; and that *Skoda*, though admitting (of course on the authority of others) the occasional association of the crepitating râle with hæmorrhagic infiltration, yet avers that he has always hitherto observed *some other kind of rale*. So that, *practically*, we may almost ignore *pulmonary apoplexy* as enlarging the list of diseases, which diminishes the semeiological value of the crepitant rhonchus as a special sign of *pneumonia*.

3. *Pulmonary Phthisis*. Unless there be coincident pneumonic infiltration, or active localized congestion in the immediate vicinity of the tuberculous deposit, a well-defined, veritable crepitant rhonchus is scarcely observed in simple tuberculosis of the lung. In acute phthisis, we sometimes discover about the apices, and at the very end of inspiration, a few distant and isolated cracklings—usually larger than the typical crepitations of the crepitant rhonchus ; but the latter, with its characters perfectly developed, must be very rarely, if ever, the product of mere tuberculous infiltration of the pulmonary parenchyma.

4. *Congestion of the Lungs*. Pulmonary congestion, whether *active* or *passive*, is almost always a secondary affection, complicating the career of other diseases. Indeed, as Swett states, we cannot speak of it "as an *independent* form of disease" (op. cit., 183). We know of no author who regards it as such, with the exception of Fournet, who, although occupied for a long time in clinical investigations on auscultation in the hospitals of Paris, and carefully noting down the results furnished by it in each new case that came into the wards, could only find 23 cases of *active sanguineous congestion of the lungs*, which he could regard as a disease distinct and different from that hypostatic infiltration which occurs in the depending parts of those organs, when any obstruction to the pulmonary circulation arises ; and in 11 of these, it ended in *pneumonia*, which, he states, is one, and the most common, of its natural modes of termination. *One third* of the cases, also, were preceded by catarrhs, which may have left traces of congestion behind (*a*). Excluding the 11 cases in which the group of symptoms that attracted Fournet's attention immediately preceded attacks of pneumonia, and (to

(*a*) Op. cit., 127, *et seq*.

use his own words) "*were the heralds of its approach,*" and which, doubtless, announced its first stage; and *one third* that had suffered from catarrhs, and we have four cases that appeared to have been produced by "long-continued efforts, asphyxia by carbonic acid gas, and long exposure to a burning sun." So that, when we come to analyze the evidence furnished by Fournet himself, only four (perhaps five) cases could be mustered, which seemed to show that there exists, as an idiopathic disease, an essentially *active congestion of the lungs,* "resulting from a superabundance of blood, from an increased energy in the forces that circulate it, and from a vicious direction given to those forces." And even this so-called distinct disease, alleged to be independent of every other, having its peculiar course, duration, symptoms and modes of termination, does not, according to Fournet, give rise to veritable crepitant rhonchus; but it is indicated, he says, by a "*humid rhonchus with continuous bubbles,*" that are more voluminous than those of the primary crepitating rhonchus of pneumonia, inconsiderable in number, three or four only being heard in each inspiratory movement, and particularly distinguished by the slowness of their development, while those of the rhonchus of pneumonia, on the contrary, as is well known, succeed each other in puffs and with precipitation.

Pulmonary congestion, however, both *active* and *passive*, occurs as an inter-current affection in the course of other diseases; and it is admitted, as we have already seen, that, under these circumstances, crepitating râle is occasionally met with; but, as Barth and Roger very correctly observe, its bubbles are most frequently larger, moister, and nearly continuous, in which case it would rather deserve the name of sub-crepitating. And add to this, that the *passive* engorgement occupies the slanting portion of the lung,—that the rhonchus follows, in regard to its seat, the same physical law, and that it is remarkable for its continuance, on account of the long duration of the disease; and, moreover, that whether the congestion be *active* or *passive*, the râle is neither accompanied nor followed by bronchial blowing, unless regular hepatization of the lung be superadded (*a*), and

(*a*) Practical Treatise on Auscultation, 96, '7.

that the vital symptoms of pneumonia are wanting, and there remains small danger of an error in diagnosis.

5. Unquestionably, the greatest *practical* difficulty and danger of mistake arises in certain cases of *capillary bronchitis*, and which misled, in the earlier periods of auscultation, the patient and philosophic mind of Andral. We are not aware, as previously stated, that it is taught by any of the most eminent and authoritative auscultators of the present day, that a perfectly-defined, true crepitant rhonchus ever occurs in simple *capillary bronchitis*; but occasionally doubts may arise in certain cases of pneumonia in which the precisely characteristic crepitation does not occur, and in some cases of *capillary* bronchitis in which very fine sub-crepitant rhonchi resemble more or less closely the veritable primary crepitation of pneumonia. But even in these cases, which are not by any means of every day occurrence, none but the lazy and careless, or those who have little, if any, *educated*, experience as *practical* auscultators,* need go astray, if due attention be paid to concurrent phenomena, and the now well-established rules for the discrimination of the different râles, which may be found in the best modern treatises on Auscultation. We will only state, *en passant*, that the true *crepitant râle* of pneumonia is, in doubtful cases, particularly distinguished from the *fine sub-crepitant râle* of acute capillary bronchitis by the universally admitted fact that it exists, in the vast majority of cases, only on one side of the chest, double primary pneumonia, as is well known, being very rarely met with; and even when it does occur, the two lungs are scarcely ever simultaneously invaded, one being attacked almost always several days in advance of the other; while, on the other hand, the sub-crepitant rhonchi of capillary bronchitis, *as a rule*, occupy both sides, and generally similar portions of the chest, and are

* This statement would seem to be too sweeping, inasmuch as Skoda alleges (Op. cit., 156), that no distinctive line can be drawn between crepitating, sub-crepitating, and mucous râles," had not this author positively affirmed, in a preceding page (viz., 150), that the *crepitant*, or, as he chooses to call it, "the fine equal bubbling râle, is the ONLY râle audible for any length of time *during inspiration only, and continuing after the patient coughs*"—thus unwittingly contradicting and nullifying, in advance, his own heterodox assertion.

diffused over a larger surface, instead of being localized, as is the crepitant râle of pneumonia, and they persist from day to day unchanged; for it is of purely exceptional occurrence that, in the capillary bronchitis of the *adult*, pneumonia is superadded. Moreover, pneumonia is a much more common disease than *capillary* bronchitis, *par excellence*. We scarcely meet with more than one case of the latter in a year, while we usually see from fifteen to thirty-odd of the former. This difference, also, exists to a marked extent even in children. Thus, while Dr. J. Forsyth Meigs observed fifty cases of well-marked lobar pneumonia among children, during a certain space of time in his private practice he met with only fifteen of *capillary* bronchitis; and, in proof of the accuracy of his statistics, it may be stated that, of all of these he kept more or less copious notes.

As sometimes offering occasion for fallacy, and of leading to the diagnosis of pneumonia, when, in fact, no disease at all exists, it may be well to mention that, after an individual has been resting for some time in the recumbent posture, and breathing quietly, we may occasionally recognize, at the end of the first few forced inspirations, a crepitant rhonchus almost exactly resembling the true primary crepitation of pneumonia; but after a few inspirations, usually two or three, it can no longer be discovered, it matters not how long, and however minutely the chest may be examined. M. Louis has the credit of being the first to direct attention to this phenomenon, which must have been observed by all auscultators much accustomed to physical exploration of the lungs. It appears to be probable, says Dr. Hughes, that it "may arise from the unfolding, or the separation, of the delicate walls of some pulmonary vesicles, which, from not being used in ordinary respiration, may have become somewhat dry, and may thence, like a dried bladder, give rise to the sound upon their insufflation" (*a*). It need hardly be stated that, to be aware of this possible source of error, is to avoid all liability of being led into a mistake by it.

So that, after all,—notwithstanding what has been said about

(*a*) Clinical Introduction to the Practice of Auscultation, &c., by H. M. Hughes, M. D., second American edit., 189.

the occurrence of the crepitant rhonchus in other diseases,—or a râle so nearly resembling it in some few cases as to be undistinguishable by its auscultatic characters alone,—it results from this *résumé*, that the well-defined, veritable râle is observed in *pneumonia*, in its first stage, in a proportion almost infinitely greater than in all other affections combined; consequently, that even as an isolated sign, it is very generally diagnostic of that disease; and if it be interpreted in connection with the clinical history and vital phenomena of the disease, the danger of mistake amounts to just nothing at all.

It appears to us, therefore, in reviewing the ground over which we have passed, that, in his effort to disparage the value of the crepitant rhonchus as a very frequent and highly important sign of pneumonia, whether that disease presents itself in its usual simple form, or as reflecting the impress of the periodic fever element, Dr. Manson has wandered widely from the path into which Science has taught the modern race of physicians to walk. It is true that, after having labored zealously to undermine and discredit it, he is, finally, constrained to admit that, "*when* present, its importance should not be undervalued, as, when taken in connection with other signs and symptoms, it affords a valuable aid to diagnosis;" yet, still, this reluctant *salvo* comes (as it seems to us) with a bad grace, if not with small weight, from one who had previously striven so hard to impress his readers with the force of his own conviction —which he "*felt it his duty*" to express — that, in "*malarial* pneumonia," at least, this râle "*is not usually present at the commencement, or, any of the stages of the disease*," and that it has no "pathognomonic value," or "any very important signification in estimating" either its "extent" or "precise seat." Indeed, does not the mere statement of Dr. Manson's first bald proposition, when viewed in connection with its subsequent tributary qualification, involve a palpable solecism almost in terms? for if, as alleged by him, the crepitant rhonchus is absent in "the vast majority of cases" of "malarial pneumonia"—that is to say, has no special relation with the natural history of the affection—and, even when present, IT has no peculiar value, and does not possess of itself

any very important significance in denoting "the extent, precise seat or gravity of the disease," how can it "afford a valuable aid to diagnosis"?

We have now concluded our survey of Dr. Manson's account of the physical signs of the *first stage* of his "malarial pneumonia." We regret the length to which the plan of our Review has necessarily led us; but we deemed it essential to a fair and full consideration of the subject, to quote, *in extenso*, many of the authorities to which Dr. Manson has only indicated the reference, as sustaining his positions; and to contest the validity of those positions, and their claim to the credence of the profession, in so far as Dr. Manson has thought it sufficient to rest them simply upon the basis of the deductions of logic. Had we been content merely to have stated in reply that the authorities upon which he relied, for the corroboration of his assertions, did not warrant any such use of their views; and that his argumentation almost invariably involved a *petitio principii*, and assumed the very points at issue; our naked asseverations would have stood alone and unsupported against his confident contra-statements. Now that the facts are before the reader, he can impartially judge between us.

We will now proceed to take up Dr. Manson's account of the *physical signs* of the *second stage* of "malarial pneumonia." These will not detain us so long as did those of the *first stage*, since Dr. Manson has offered but little, under this head, as the result of his own experience and reflections. The following is all that is presented by Dr. Manson on the subject in hand.

"SECOND STAGE, SOLIDIFICATION, HEPATIZATION.

"These terms are synonymously used to denote that condition of the lung, in pneumonia, in which it is no longer permeable to the air, but, doubtless, are too often indiscriminately used. In this affection, as we have repeated and shall endeavor to maintain, the state of the lung parenchyma partakes more of the character of congestion than active inflammation. There can be but little doubt but that, in a great proportion of cases, *some degree* of inflammation exists, as proven by the presence of fixed pain—the peculiar sputa and other symptoms; but often, as we have premised, the expectoration is of a different character, partaking more of the appearance of diluted blood, or of a sero-sanguineous nature.* When, therefore, perfect dullness

* In this disease the expectoration is usually an exhalation, and not the result of inflammation.

on percussion, and absence of the respiratory sounds, betoken condensation of the lung in this disease, it is not, in my opinion, due to an inflammatory process with lymph effusion, as in the frank form of pneumonia, and to which the term hepatization has been properly applied. We shall endeavor to substantiate these views when the nature of this affection is considered. Lungs, says Walshe,* which the naked eye would judge to be in the same condition in regard to their shares of air, fluid, and semi-plastic substance, it is evident, acoustically, are in different physical states: specimens of parenchyma apparently identical, are in reality widely different. In estimating the conducting power of lungs, there can be but little doubt that varying homogeneousness is a more important element than any so-called 'solidification in pneumonia.'

"'Dullness,' says Blakiston,† 'will result from serous or sanguineous congestion of the cellular portion of the lungs in contact with the walls of the thorax, whether of a passive character, being the result of obstruction to the circulation, or of an active character, as in the early stages of pneumonia.'

"Andral‡ considered hepatization to arise from congestion of blood, and not from lymph effusion, and in this opinion Stokes also concurs. Blakiston,§ we think, very reasonably concludes, and cites experiments in proof, that in pneumonia there are two forms of solidification, one of which is produced by congestion, and the second by deposition of lymph.

"*Percussion Signs of Solidification.*—'The thoracic walls,' says Skoda, 'beneath which the hepatized lung lies, yield a dull percussion sound, and their resistance is increased, provided the hepatized portion of the lung has the thickness of about an inch, and an extent greater than that of a plexi-meter. The more extensive the hepatization, the duller is the sound, and the greater the resistance. If the sound is extremely flat, as elicited in striking the thigh, then the lung is solidified throughout its whole thickness.'

"*Auscultatory Signs.*—According to Lænnec, in hepatization the crepitant rhonchus and vesicular murmur are absent in the affected part, and this is frequently the only proof afforded of solidification having occurred, broncophony existing in certain cases if the inflammation is seated near the roots of the lungs, or in the superior lobes where the bronchial tubes are largest. When the pneumonia is central, broncophony is absent or obscure, becoming more manifest if the inflammation approaches the surface of the lungs; this being easily accounted for, he observes, as a hepatized lung is a better conductor of sound than a healthy one, bronchophony being nothing more than the resonance of the voice within the bronchi of the inflamed part. Bronchial respiration and bronchial cough, according to the same author, always accompany broncophony, and when the hepatization is near the surface and involves within it bronchial tubes of considerable size, then it is frequently accompanied by a sensation of blowing into the ear (*souffle dans l'oreille*), and if a thin portion of pulmonary substance not yet hepatized

* Prac. Treat. Heart and Lungs, 131.
† Prac. Obs. Dis. Chest, 37.
‡ Path. Anat., vol. i., 511.
§ Dis. of Chest, 286.

intervenes between the ear and the affected bronchi, the sensation denominated the veiled puff (*souffle voile*) is produced! The auscultatory sounds, as described by Skoda, more nearly accord with my limited experience. I, however, do not feel competent to pronounce on the correctness of his theory of consonance. He says: 'Where the hepatized portion is extensive enough to contain within it at least one of the larger bronchial tubes, and if the tube contains air, is not obstructed, and communicates with the trachea, then the voice of the patient will consonate in the bronchial tube, and be heard either as loud or weak bronchophony over those parts nearest the tube.' 'The respiratory sounds vary according to the nature of the râles in the larynx, trachea or bronchii through which the air must pass before it enters the bronchial tube surrounded by the hepatized tissue. The respiratory murmur may alone be heard, or in addition to this, hissing, whistling and sonorous sounds, either separately or combined. If the hepatized portion of the lung be not large enough to contain one of the larger bronchial tubes, or if it be large enough, but the tube be filled with fluid or solid matters, or if the communication between the tube and trachea be obstructed by mucus, blood, &c., then neither bronchophony nor bronchial breathing, nor high, clear râles, nor hissing, whistling or sonorous sounds will be heard over the hepatized lung; the patient's voice will be *inaudible*, or recognizable only as a dull muttering, and the respiratory murmur *indistinct* or *absent*.' Whether the explanation of Skoda is correct or not, I cannot say, but certainly, in a large majority of my cases, neither bronchophony, bronchial breathing, or bronchial cough, have been observed. I am inclined, however, to attribute their absence to the fact that, in this variety of pneumonia, the impermeability of the lung being due to excessive engorgement, and not to lymph exudation, it therefore does not possess the requisite density to make it a good conductor of sound."

Passing by, for the present, Dr. Manson's criticism upon the terms "*Solidification*" *and* "*Hepatization*," we will now direct attention only to the *physical signs*.

1. The "*Percussion Signs.*" It will be observed that Dr. Manson gives no experience of his own in respect to the results furnished by percussion in "malarial pneumonia," farther than the statement that "perfect dullness on percussion, and absence of the respiratory sounds, betoken condensation of the lung in this disease." He contents himself with making quotations from Blakiston and Skoda; and yet, if we examine the views of these authors in this regard, we shall find that they differ very widely. Thus, while *Blakiston* teaches that dullness on percussion may result from simple serous or *sanguineous congestion of the lungs*, whether that congestion be of an active character, as in the early

stages of pneumonia, or be caused by mere obstruction to the circulation; *Skoda* teaches that "the percussion sound remains *unaltered, however congested with blood* the vessels of the lung may be, so long as *no exudation* has taken place into the lung parenchyma;" and adds, that "the *truth* of this is *demonstrated*, not only by observation of pneumonia in its first stage, but also, and more strikingly and conclusively, in cases of constriction of the mitral orifice of the heart, where the *highest degree of congestion* of the pulmonary vessels takes place, without producing *any change* in the normal percussion sound of the lungs" (*a*). So that, so far as these authors are concerned, the statements of the one are completely contradicted and nullified by the statements of the other. The quotation from Skoda seems particularly unfortunate; for while *Dr. Manson*, on the one hand, states that "perfect *dullness in percussion, etc.*, betoken condensation of the lung" in "malarial pneumonia," and that this "condensation of the lung" is "*not*, in his opinion, due to an inflammatory process with lymph effusion, as in the frank form of pneumonia—repeatedly and confidently affirms, as a matter of "*fact*, that in this (malarial) variety of pneumonia, the impermeability of the lung is due to *excessive engorgement*, and *not* to lymph *exudation*;" Skoda, on the other hand,—whose descriptions are confessedly drawn from the observation of cases in which the hepatization *is* due to exudation of lymph,—affirms with equal confidence, as a "*demonstrated truth*," that, in pneumonia, the "*percussion sound* remains *unaltered*, however *congested with blood* the vessels of the lung may be, so long as *no exudation* has taken place into the lung parenchyma." Moreover, as we have seen in the foregoing pages, and had occasion several times to remind Dr. Manson, Skoda ignores the very existence of the division of pneumonia that he describes—states positively that it "*is altogether based upon theory*"; and even were this variety of pneumonia admitted by Skoda, he would be compelled to reject *Dr. Manson's* pathology of it, or deny, *in toto*, the appositeness of Dr. Manson's quotation from his work, or confess that said quotation itself is not based, as he declares it is, upon "*truth*."

(*a*) Op. cit., 304.

2. "*Auscultatory Signs.*" Dr. Manson says that the auscultatory sounds of this stage, as described by Skoda, more nearly accord with his experience than those described by Lænnec. Dr. Manson, however, fails to point out clearly in what respect Skoda's description of these phenomena more nearly accords with his own experience than that given by Lænnec. But from his statement that, "in a large majority of my (his) cases, neither broncophony, bronchial breathing or bronchial cough have been observed"; and from his italicizing that part of the quotation from Skoda in which, in consequence of certain specified causes, Skoda states that "the patient's voice will be *inaudible* or recognizable only as a dull muttering, and the respiratory murmur *indistinct* or *absent*"; we infer that Dr. Manson deduces the conclusion from Skoda's language, that he, also, not unfrequently failed to observe the phenomena of *bronchophony and bronchial breathing* in pneumonia, throughout its career, when the inflamed lung-tissue is impermeable to air—*hepatized.*

In order to a more intelligent appreciation of the subject, perhaps, it may not be inappropriate to state briefly the several theories propounded by Lænnec and Skoda, in explanation of the phenomena in question. For the purpose of illustration, we will take *bronchial respiration.*

According to Lænnec, in the normal condition of the lungs, the natural *bronchial breathing* is not transmitted to the ear in consequence of the sound being stifled by the vesicular murmur. If, from any cause, the vesicles be deprived of air, and the lung-substance becomes condensed or solidified, then, owing to suppression of the vesicular murmur, and the increased conducting power of condensed lung, the current of air in the larger bronchi of the condensed or solidified lung is transmitted to the ear enhanced in intensity in a proportion directly dependent upon the extent and degree of consolidation, and the size of the tubes in the condensed lung.

Skoda alleges, on the other hand, that sound is heard somewhat further through healthy than through hepatized lung, and endeavors to explain the phenomenon in question upon the musical principle of consonance. The air in the bronchial tubes,

according to him, consonates with the tracheal and laryngeal sounds in the same manner that a guitar-string yields a musical note, when a similar note is sounded upon another instrument in its neighborhood. But, as the force of consonance depends upon the form and size of the enclosed space, and upon the nature of the walls forming it; and the more solid the walls, the more completely will the sound be reflected, and the louder the consonance; and as the bronchial tubes pass into the parenchyma of the lungs, their cartilaginous structure gradually disappears, and finally the smaller bronchi are merely their membraneous canals; in order that increased consonance of the voice shall occur in the bronchial tubes which run into the parenchyma of the lungs, it is requisite that their walls must be cartilaginous, or, if membraneous, the membrane must be very dense, or the *surrounding pulmonary tissue must be deprived of air:* thus increasing, in any of these cases, the sound-reflecting power of the tubes. Of course, this theory requires for the development of the sounds produced by consonance, that the communication between the air in the tubes, and the air in the larynx, must remain uninterrupted.*

Now, without entering into a discussion of the merits of these several theories, we will simply look at the practical fact which both of them seek to explain. In order that *bronchial respiration* and *bronchophony* shall exist in pneumonia, both theories require, as a necessary condition of their observation, that the air-cells be deprived of air, and the tissue of the lung be condensed or hepatized. It is, therefore, clinically unimportant which theory be correct; for when the phenomena in question are observed in pneumonia, they indicate beyond doubt that the *lung-tissue is impermeable to air*—that is, the lung is *hepatized.* What if,

* We would refer the reader, curious in such matters, to the works of Walshe and Flint (so often referred to in these pages), for a full and able discussion of the theories of Lænnec and Skoda. For ourselves, we cannot help thinking with Walshe, that, if the theories of the immortal discoverer of Auscultation do not, in all instances, bear the test of clinical scrutiny, those of his German critics seem neither to be always in harmony with the principal of acoustics, nor accordant with the results of actual experiment. Although the true principles involved in the mechanism of auscultatory phenomena are interesting topics of inquiry, fortunately for diagnosis, they are clinically of little comparative importance.

according to Skoda, the presence of mucus, blood, etc., in the bronchi should prevent their production, all that one has to do to elicit their evolution is, to remember what he says, and to test it by actual practice. "Obstructions to the free communication between the bronchial tube," says he, "caused by the presence of fluid or solid matters in the tube, may be *removed* by *coughing*, or by *expectoration;* thus, after coughing or expectoration, *bronchophony*, *bronchial breathing*, consonating râles, etc., *frequently manifest themselves*, *in cases where previously they had not been heard*" (Op. cit., 311). Skoda, however, fails to state, in describing the signs of *hepatization*, how often the observance of *bronchial respiration*, *bronchophony*, etc., over the hepatized lung, is prevented by the occurrence of the conditions assigned by him as interfering with their evolution, and causing the voice of the patient to be inaudible or recognizable only as a dull muttering, and the respiratory murmur to be indistinct or absent; and here we have a striking illustration of a prominent fault that pervades this author's entire work, and which makes it so untrustworthy a guide for the beginner.* Instead of clearly describing, and duly dwelling upon, the physical signs that are developed in accordance with the natural pathological laws of a disease, and then pointing out such as may sometimes occur in the course of its career, he mentions and mingles together, without adequate discrimination in respect of their clinical frequency and special importance, almost every possible variety of physical signs that casual circumstances, or an accidental complicating affection, may impart to it. Consequently, the mind of the student is not only perplexed with the description of a great variety and confusion of sounds, and is unable to separate those that particularly belong to the diagnosis of the disease in hand, but may, also, be led into the grave practical error of magnifying the occurrence of really infrequent and purely incidental phenomena, and of under-estimating the frequency of those that are of habitual occurrence and of distinctive value. He is wholly

* His very partial translator, Dr. Markham, is constrained to admit that the work of Skoda is defective as a hand-book of details for the student, and states that he once had an idea of attempting to fill up what is wanting.

at a loss to know what signs are to be regarded as diagnostic of any given affection, and what he ought to look for at the bedside as indicative of it. Indeed, from Skoda's description of the auscultatory signs of pneumonia, when the lung is *hepatized*, it would seem that even so intelligent and experienced a physician as Dr. Manson has deduced the conclusion that, in this condition of the lung, Skoda frequently failed to discover, throughout the career of the disease, *bronchial breathing, bronchophony and bronchial cough*, and that the patient's voice was *inaudible*, or recognizable only as a dull muttering, and the respiratory murmur *indistinct* or *absent*, and yet Skoda could not have intended to make such an impression; for, in a previous chapter, he had stated that these signs are fluctuating; that the "alternate *presence* and *absence* of the thoracic voice at the same part of the thorax, *when the lung is hepatized*, is a well-known and common occurrence"; and that "every one who has had much experience in the auscultation of pneumonia must have observed the increased voice, *bronchophony*, to *appear* and *disappear* several times in the *course of a few minutes*"; and, as we have already seen, he explains the alleged intermission of the phenomena in question by the presence of mucus, blood, etc., obstructing the bronchi, and which (he says) may be removed by a "deep-drawn breath, and still more readily if the patient coughs" (*a*). It can scarcely be necessary to remark, then, that Skoda's experience in regard to the frequency with which the physical signs of hepatized lung—*bronchophony, bronchial breathing and bronchial cough*—were recognized by him, must have differed very widely from that of Dr. Manson, who affirms that, certainly in a large majority of his cases of that variety of pneumonia which is generally seen by Southern physicians, neither of these phenomena have been observed. Dr. Manson's very singular experience, therefore, is not at all verified and sustained by the observations of his favorite—the Viennese Professor; and even the experience of the latter in respect to *bronchial respiration* and *bronchophony* being fluctuating signs in pneumonia,—appearing and disappearing several times in the course of a few minutes,—is

(*a*) Op. cit., 68. '9.

not, to the extent stated by him, borne out by other practical auscultators, highly distinguished for their accuracy and acuteness. Walshe says that since he first became acquainted with Skoda's statement concerning the appearance and disappearance of *bronchophony* in hepatization, he has repeatedly endeavored to produce artificially the change Skoda describes, and that as *a rule* the experiment has wholly failed; in some *rare* instances a very slight passing change in the force of resonance may be perceived (*a*). And Flint has put the allegations of Skoda to the severe test of careful clinical scrutiny, and accurately noted down the results. He affirms, as the result of his observations, that, when once developed, *bronchophony* is a persistent sign until it disappears as the consequence of the progress in resolution; that is, it is generally found at each successive examination; and proves his statement to be correct by an analysis of a series of recorded examinations. Of eighty-eight examinations in twenty-seven cases, in but two instances was this sign absent when its existence was noted at the examination preceding, and also that succeeding the one on which it was found to be wanting. As regards the *frequency of its occurrence* in pneumonia, he states that of twenty-seven cases just referred to, in the histories of which is noted either the presence or absence of this sign, it was observed in twenty-five, and not discovered in only two; and in these two cases it might have existed and disappeared, since the explorations were mostly made during the latter part of the disease (*b*).

In regard to *bronchial respiration*, Flint makes the following statement: "The bronchial respiration in acute lobar pneumonitis, is not a variable or fluctuating sign. *As a rule*, after it is developed, it may be discovered at each successive examination, until, in the progress of the disease, it declines and disappears. There are, however, occasional exceptions to this rule. I have known it to be absent and shortly reäppear, its temporary cessation being perhaps due to casual obstruction of the tubes. Such obstruction, during the period of the disease when the bronchial respiration may be expected to be present, *rarely* occurs in ordi-

(*a*) Op. cit., 143. (*b*) Op. cit., 416, 418.

nary cases of pneumonitis. During the progress of the disease in forty cases, the bronchial respiration existed in one hundred and seven out of one hundred and forty-six examinations made on different days. Of the remaining thirty-nine cases, in seven there was absence of respiratory sound, and in thirty-two the modification came under the denomination broncho-vesicular. These enumerations show the persistency of this sign in cases of pneumonitis." In respect to the *frequency* with which this sign occurs in pneumonia, he states: "The bronchial respiration is absent in but an *exceedingly small proportion* of instances. Of the forty-five cases which I have selected for analysis, commencing with the last case recorded, and rejecting none till this number was completed, in five either the examinations were begun too late in the disease, or the records are imperfect with respect to this point. Excluding these five cases, out of the remaining forty the *bronchial respiration* was more or less marked in *thirty-seven*. In two the modification did not exceed that constituting the broncho-vesicular respiration; and in the other exceptional case the patient died on the second day in the stage of engorgement, the disease being complicating with dilatation of the heart. In the large collection of cases analyzed by Grisolle (373), the *bronchial respration* was observed to cease two days before death in one, and was not developed in another of two cases in which the inflammation extended over an entire lung; and of the cases in which the inflammation was limited to a single lobe, it was wanting in nine"(*a*).

That *bronchial respiration* and *bronchophony* are the auscultatory signs by which, in ordinary lobar pneumonitis, we determine that the lung is *hepatized*, and that they may be recognized in the immense majority of cases, are facts so well known, and so universally acknowledged at the present time, as not to admit of question (*b*). No facts whatever can be better estab-

(*a*) Op. cit., 412, '13.

(*b*) Gerhard, Dis. of the Chest, 138; Stokes, Dis. of Chest, 278; Andral, Med. Clinic, vol. ii., 188; Hughes, Aus. and Percus., 159, 203; Barth & Roger, Treat. on Aus., 67, 126; Walshe, op. cit., 415; Swett, Dis. of Chest, 101; C. J. B. Williams, Cyc. Prac. Med., vol. iii., 604, 605; Grisolle, op. cit., 237–241; Watson, Prac. Med., 565; Tanner's Manual Clin. Med., 2d edit., 185; Barlow, Prac. of

lished, or more completely placed beyond all doubt or cavil. Dr. Manson's experience in "malarial pneumonia," then, is most extraordinary. He could not discover either of these phenomena in a *large majority* of his cases; and is inclined to attribute their absence to an assumed excessive engorgement of the lung, independent of lymph exudation, which, says he, does "not possess the requisite density to make it a good conductor of sound." But of the nine cases of pneumonia seen by Grisolle, in which the inflammation was limited to a single lobe, and over which no sound, either normal or pathological, could be recognized, there were *seven* in which the most careful examination failed to reveal any particular conditions capable of explaining the anomaly observed during life; the bronchi were split up and explored as far as fine scissors could penetrate, and found to be pervious, and there were only two cases in which the diseased lung presented a flaccid and softened appearance, resembling the *splenization* of the lung in certain cases of typhoid fever (*a*). And Walshe states that "in certain rare cases of *pure red hepatization*, the physical signs, with the exception of dullness under percussion, are wholly wanting; the vocal fremitus is deficient; there is neither vocal resonance nor respiration heard on the affected side" (P. 416). It does not necessarily follow, therefore, that, because Dr. Manson could hear neither *bronchophony*, *bronchial breathing*, *nor bronchial cough*, in a large majority of his cases of "malarial pneumonia," the lungs, in those cases, did not possess the requisite density to be good conductors of sound, nor that the impermeability of the lung was "due to excessive engorgement, and not to lymph exudation." It has doubtless occurred to the reader in this connection, that although Dr. Manson did "not feel competent to pronounce on the correctness of Skoda's theory of consonance," yet, he utterly rejects this writer's views in regard to sound passing more readily through healthy than through hepatized tissue; for, while Dr. Manson alleges that the lungs are not dense enough, in "malarial pneumonia," to

Phys., 191; Wood, Prac. Med., vol. ii. 12, 13; Copland, Med. Dict., vol. ii. 884; Dunglison, Prac. Med., vol. i. 305; Dickson's Elements Med., 606, &c.

(*a*) Op. cit., 241.

be good conductors of sound—and, *a fortiori*, in the normal state they cannot be sufficiently so,—Skoda, on the other hand, would say that, for the very reason assigned by Dr. Manson—the alleged want of density of the lungs in the pneumonia that he saw—the pulmonary parenchyma was in a better condition for the conduction of sound than if it had been more solid, since, according to Skoda, sound is heard somewhat further even through healthy than through hepatized lung. "The difference in this respect," says he, "is remarkable" (P. 73).

Recurring to the physical signs of the *second stage* of "malarial pneumonia," as given by Dr. Manson, let us see what they are? As the result of Dr. Manson's own observations, we have presented as the physical signs of the *second stage* of that form of pneumonia which is generally met with by Southern physicians, simply, "*perfect dullness on percussion, and absence of the respiratory sounds*"; for, says he, "certainly in a large majority of my cases, neither bronchophony, bronchial respiration, or bronchial cough, have been observed"! Now, exclusive of certain *rare* cases of *hepatization*, what are these but the physical signs of an extensive accumulation of fluid in the sac of the pleura; and, as hydrothorax is scarcely ever observed as a primitive affection, being incident to structural lesions of the heart and kidneys, do not the phenomena described by Dr. Manson as indicative of condensation of the lung in "malarial *pneumonia*," almost always point out, in an acute idiopathic disease, the existence of simple *pleurisy*, with large effusion? Indeed, even in pleurisy, and when the accumulation of fluid has become sufficient to produce dilatation of the side and thoracic displacements, although all respiratory sounds may be entirely suppressed generally over the affected side, yet, as is well known, we may often detect along the spine, and especially over the root, and at the apex of the lung, a rude, bronchial, or slightly blowing quality of the respiration, and, in many cases, diffused blowing respiration is pretty extensively heard over the diseased side; while in cases of moderate effusion, the respiratory murmur may be generally heard weakened and deep-seated over the site of the effusion, and we may also often hear, towards the

upper limit of the fluid, and particularly between the lower angle of the scapula and the vertebræ, bronchial respiration and bronchophony more or less marked. (*a*) And in *pneumonia*, it is admitted, on all hands, that the physical conditions requisite for the development of bronchial respiration and bronchophony are usually better fulfilled than in any other disease of the lungs, the pulmonary parenchyma being often perfectly consolidated without compression of the tubes; and bronchial respiration is frequently so loud as to be designated *tubular*, and to acquire a metallic character to such a degree that, as Walshe observes, the sounds may, without exaggeration, be compared with those produced by blowing sharply through a brass tube: so that tubular and pneumonic breathing may be used as convertible phrases. It can scarcely be necessary to add, that bronchophony is a coincident phenomenon.

Surely, then, Dr. Manson's announcement that, in the second stage of a large majority of cases of "malarial *pneumonia*"—which, according to him, is the form of pneumonia generally seen at the South—the almost constant phenomena of solidified lung, *bronchial breathing*, *bronchophony*, *and bronchial cough*, have not been observed, must be regarded as rather a startling statement. Other observers seem to have been rewarded with more positive results, when they have met with pneumonia stamped with the impress of a malarial or periodic fever element. While reviewing Dr. Manson's account of the physical signs of the first stage of his "malarial pneumonia," we had occasion to cite Professor Wood as stating that, in this form of pneumonia, the physical signs offer no material variation from those that characterize the uncomplicated affection; and we also cited Dr. Drake as affirming that, when pulmonary inflammation is developed in connection with intermittent fever, this inflammation "will be indicated by cough, dyspnœa, pain, and the *ordinary auscultatic*

(*a*) Walshe, op. cit., 354–'6; Flint, op. cit., 546, '7; Grisolle, op. cit., 508; Gerhard, op. cit., 78, '9; Swett, op. cit., 137; Hughes, op. cit., 171–'4; Skoda, op. cit., 350, '51; Andral, op. cit., vol. ii. 402; Watson, op. cit., 583; J. B. S. Jackson, Amer. Jour. Med. Sciences, vol. xi. N. S., 16 *et seq.*; Barlow, op. cit., 213; Wood, op. cit., vol. ii. 40; Stokes, op. cit., 459, '60.

signs." In speaking of the physical signs of the second stage of a form of pneumonia which he regarded as of malarial origin (and which certainly was pneumonia that prevailed in the South), Dr. Ramsay, of Georgia, states: "*Bronchial respiration is generally quite audible*"; and "*bronchophony is also a feature of this disease.*" (*a*) Of the 103 cases occurring in India upon which Dr. Morehead based the remarks embraced in his chapter on Pneumonia, seventy-six were of primary pneumonia, and twenty-seven were of pneumonia complicating intermittent or remittent fever. In treating of the physical signs, he says that it is unnecessary to enlarge on a subject now so well understood as the physical signs of pneumonia; but on this point he "would merely observe, that *bronchial respiration*, with some degree of crepitus in the adjacent parts, and presence of the vocal thrill, were the signs held to indicate the existence of the second stage" (*b*); clearly implying that, according to his experience, the physical signs were identical in the two forms of pneumonia described by him, and that they are the same everywhere. In his paper on Pneumonia, already referred to in another connection, Dr. Ames, of Montgomery, Ala., states that it is not uncommon for pneumonia in his latitude to present "marked dullness on percussion, *bronchial respiration, and bronchophony* in some part of the diseased structure, sometimes over a large part of it, within the first twenty-four hours." (*c*) Now, if the phenomena in question be so commonly observed in the pneumonia of the South, they must be commonly recognized in "malarial pneumonia," or "malarial pneumonia" is not, as Dr. Manson asserts it is, "the form of pneumonia generally seen by Southern observers."

We pass on to Dr. Manson's account of the *third stage* of "malarial pneumonia."

"THIRD STAGE—PURULENT INFILTRATION."

"This is a very rare termination in this form of pneumonia. When present, the physical signs do not differ from those of solidification. A well-

(*a*) Charleston Med. Journal, vol. vi. 8.
(*b*) Researches on Disease in India, vol. ii. 338.
(*c*) N. O. Med. and Surg. Journal, vol. x. 423.

marked case of this condition came under my care, which was attended with copious purulent expectoration tinged with blood, being of a yellowish pink hue, the quantity evacuated being at least two pounds per day for several consecutive days. Laryngeal, tracheal and bronchial râles were present and distinctly heard at a distance of several feet from the patient. Percussion yielded a dull, flat sound over the whole thorax below the fourth rib, on each side, beneath which no sound of the voice, cough or respiration could be heard. Above this line percussion yielded a clear sound, and the ear discovers a coarse, humid rhonchus with large bubbles, evidently due, like the sounds in the windpipe above, to the passage of air through the abundant purulent secretion."

Dr. Manson states that the *third stage* of *purulent infiltration* is very rare in "malarial pneumonia"; and so we should think it must be, since purulent matter is one of the products of inflammation, and, according to our author, "the *anatomical character* of this affection is doubtless an *intense congestion* of the parenchyma, associated, *perhaps*, in *some* degree, with inflammation." But as there are no evidences during life by which the third stage of pneumonia can be certainly distinguished from the second, it is not easy to say, in the absence of *post mortem* examinations, carefully conducted by competent pathologists, whether the former stage be very rare in "malarial pneumonia" or not. It does not appear, however, from his essay, that Dr. Manson has fortified his pathological views of the disease in hand by any such matter-of-fact test. But should he be hereafter inclined to direct his attention to the elucidation of its precise pathology by a minute examination of the lungs after death — and which we can but regard as the true touchstone and *sine qua non* of his pathological doctrines,—we would, with all suitable respect, refer him to the observations of Dr. Da Costa, whose researches prove that although the third stage of ordinary acute pneumonia is generally believed to be attended by suppuration, yet, in this stage, pus-cells are frequently absent; and the increased quantity of fat which is detected in solidified lung, and the soft, broken-down appearance of its parenchyma, seem to indicate that there is a gradual breaking up of the exudation, allied in its nature to fatty degeneration, such as occurs in other parts of the body as a result of acute inflammation. The non-suppurative nature of *gray hepatization* is confirmed by the fact,

now well-established, of the excessive rarity of pulmonary abscesses as the result of pneumonia. (*a*) It seems, however, that Dr. Manson has met with at least one well-marked case of "*malarial* pneumonia in which there was a large amount of *purulent matter* expectorated daily for several days in succession; and this occurred, too, in a disease rather of a congestive than inflammatory character!

The first reflection which forces itself upon the mind in reading Dr. Manson's description of this case is, that it should be the only one to which he particularly alludes, and of which he gives any account, in his essay, and yet that it should be so flatly contradictory of the pathology of the disease, which he seeks specially to establish. We are told all along, whenever the pathology of the affection is alluded to, that he is describing a *congestive* pneumonia, *par excellence*, in which, although "*some* degree of inflammation exists," yet, nevertheless, the "inflammatory process" is insufficient to be productive even of "lymph exudation." The case in hand, then, is, on more accounts than one, rather curiously extraordinary. We doubt, indeed, if it has a parallel. It is not easy to understand how a patient could live, for several consecutive days, with perfect flatness on percussion over the whole thorax below the fourth rib, on each side, with râles extending from the bronchi to the larynx, distinctly audible at a distance of several feet, and expectorating at least two pounds per day of pink-colored pus! And it is not a little remarkable, also, that there should have been laryngeal, tracheal and bronchial râles, owing "to the passage of air through the abundant purulent secretion;" and yet "no sound of the voice, cough or respiration could be heard" over the seat of the disease from whence issued such an enormous amount of pus! There was genuine inflammation here, certainly; and since the malarial poison did not prevent the terrible ravages of the most vehement "inflammatory process" in this most extraordinary case, we respectfully submit that there may well be a milder grade of

(*a*) American Jour. Med. Sciences, N. S., vol. xxx. 306 *et seq.*, *Dr. Gross* adopts Dr. Da Costa's views. See Elements of Pathological Anatomy, by S. D. Gross, M. D., 3d edit., 412.

inflammation in ordinary cases of "malarial pneumonia," but still sufficiently potent to end in the exudation of lymph.

Next come the

Signs of Resolution.—Dr. Manson gives no experience of his own in regard to the signs which announce the *resolution* of "malarial pneumonia," farther than the quotation, under this head, of the views of his favorite Skoda, as according with his own observations. Skoda maintains that the *period of resolution* of pneumonia does not invariably commence with the appearance of the crepitant rhonchus, but that it is attended in most cases by a great variety of râles, and, in some rare cases, that no râles whatever appear during this stage of the inflammation. Flint agrees with Skoda that the returning crepitant râle—*râle crepitant de retour*—described by Lænnec as characterizing the resolution of pneumonia, occurs only in a certain proportion of cases; and in this others, also, coincide. Of course, there is nothing in this circumstance, it can scarcely be necessary to add, at all peculiar to "*malarial* pneumonia." One cannot help deeming it as not a little singular, however, that Dr. Manson's experience in relation to the physical signs of "malarial pneumonia" should be so nearly coincident, in his opinion, with the experience of Skoda, who ignores the very existence of Dr. Manson's division of pneumonia, and declares that it is "altogether based upon theory." Skoda professes to describe the physical signs of a form of pneumonia, "the anatomical condition of the lungs" of which are "*exactly such as is described by Lænnec;*" Dr. Manson professes to describe the physical signs of a disease of the lungs, which, although called pneumonia, differs from Lænnec's form so much as to be "*unlike in character and nature*"—which is alleged to produce anatomical changes in the lungs that are "*widely different;*" and yet the physical signs of these "*widely different*" physical conditions offer the greatest and most marvellously curious resemblance throughout their career! There is a mistake here somewhere.

If we now bring together Dr. Manson's account of the physical signs of "malarial pneumonia," what shall we have?

1. "In the first stage of this disease," says he, "we may have

the crepitating râle, or mucous râles, in varying degrees, according as the character of the case may partake of inflammation or active engorgement"; but as the crepitating râle is not, according to Dr. Manson, usually present at any time during the conrse of the disease — is absent in the large majority of cases; and as the anatomical character of this affection is said (by Dr. Manson) to consist of an intense congestion, *par excellence*, of the pulmonary parenchyma, being only "associated, *perhaps*, in *some* degree, with inflammation"; the *first stage* of "malarial *pneumonia*" is habitually heralded by *mucous râles, in varying degrees;* or, in other words, the *first stage* of this so-called *pneumonia* is to be recognized, in the large majority of cases, by the physical signs of *bronchitis*, which is as much as to say that, so far as physical signs can be relied upon to indicate an affection of the lungs, the *first stage* of "malarial *pneumonia*" is, usually, a *bronchitis.*

2. The *second stage* of "malarial pneumonia," according to Dr. Manson, is betokened by "*perfect dullness on percussion, and absence of the respiratory sounds*"; as, "certainly in a large majority of cases, neither bronchophony, bronchial breathing, or bronchial cough, have been observed." That is to say, the physical signs of the *second stage* of "malarial *pneumonia*" coincide, in certainly a large majority of the cases, with the physical signs of certain cases of *pleurisy* with copious effusion.

3. The *third* stage of "malarial pneumonia" is represented by Dr. Manson to be very rare; but the physical signs of this stage do not differ from those of the *second.*

And these are the results of physical exploration which Dr. Manson has attained in that "form of *pneumonia*" which, according to him, is "*the* form of *pneumonia* generally seen by Southern observers"! For the sake of exact Diagnosis, it is to be hoped that an experience, so discouraging and peculiar, will not be verified by other observers.

(To be continued.)

REPORT ON THE DISEASES OF FRANKLIN COUNTY.

[*Submitted to the State Medical Society May*, 1859.]

BY W. C. LANKFORD, M.D.

FRANKLIN County has not suffered from an epidemic, or any disease of an endemic nature for the past twelve months; and we have encountered but few sporadic diseases which would merit the attention of the profession.

In the summer months of 1858, Diarrhœa and Dysentery prevailed to some extent throughout the county—Dysentery, as a general rule, being the result of Diarrhœa. But few cases proved obstinate, under proper treatment.

In cases of Diarrhœa, when ingesta or any irritating secretions were supposed to be the cause, a brisk but mild cathartic would be given, after the action of which, laudanum, Dover's powder, opium and acetate plumbi, acid. tannici, pulv. galls, &c., would be administered, a few doses of ether generally being sufficient to arrest the disease. If the evacuations evinced a deficiency of bile, I prescribed calomel with one of the above astringents, or equal parts of blue mass and ext. taraxicum. In Dysentery, in addition to the above treatment, I usually applied large emollient cataplasms over the abdomen. If there was much thirst and fever, ice beaten into small fragments and applied to the abdomen, in a bag of oiled-silk, gave much and sometimes immediate relief. If ice could not be procured, cloths saturated in cold salt-water would be used in lieu thereof.

When these measures failed, blisters were drawn over the tenderest portion of the abdomen, while enemæ of lead and opium, sulph. zinc, or one of some other appropriate astringent was administered: oil of turpentine is also valuable when there is tympanitis and tenderness.

Under this treatment the disease was very controllable, and of short duration.

In the Fall, Typhoid Fever was met with in some sections, but did not prevail to much extent. In the autumn of 1856, the western part of the county was visited by an epidemic of this fever, since which time the neighborhood has been comparatively free from its malignant presence. And here I will

remark, that, so far as my observation has extended—and I have heard physicians of more experience express the same—after typhoid fever has prevailed epidemically in any location, that section is apt to be remarkably free from all diseases of an epidemic nature, typhoid in particular, for several years subsequent. This is

> "That dire disease, whose ruthless power,
> Withers the beauty's transient flower";

and it should hold the undivided attention of every practitioner of medicine until he can subdue its relentless course. We find it attacking alike the wealthy and the poor, the robust and the delicate, the young and the man of mature years;—those advanced in life I think less subject to it, than those of middle age. It seizes and prostrates those whom we would least expect, independent of location, habits, vocation, or any apparent exciting cause.

Surely here is a wide field for observation and experiment, for study and untiring research—and he who discovers a specific, or introduces measures which can suddenly subdue this fever, will, like Jenner, receive the plaudits of all succeeding generations, and render his name as immortal as the hero of the Revolution. Most of the cases with which I met last Fall were of a mild type, and of three or four weeks' duration. Sometimes the subjects were taken suddenly with a chill, after which the fever would take its onward course, but more frequently the patient would feel dull, stupid and languid, and would complain of vertigo, and of a dull aching of the spine for several days previous to confinement. In the treatment I had reference to the temperament and constitution of the patient, which, if I found to be weak and debilitated from previous causes, received a treatment calculated to support and strengthen the system in the latter stages of the disease. In the commencement, that is, for the first two or three days, if there were intermissions, I administered quinine and blue mass alternately every two hours, the quinine in large doses, and the blue mass in three-grain pills, until the exacerbation came on. If this failed to break up the fever, and it should continue without intermissions, I desisted entirely from

the blue mass, deeming the intermission the only circumstance under which mercurials are admissible, and employed a refrigerating, diaphoretic treatment, meeting urgent symptoms, according to their nature, as they would arise. The disease once fairly established, would generally reach its maximum from the eighth to the twelfth day. After the twelfth, the active delirium would subside into a low muttering and confused train of ideas; the pulse would lose its frequency, and the tongue its coat, and present that red, polished aspect so characteristic of this fever. To the untutored eye, this stage would seem to be one of convalescence, and friends of the sufferer would hail it with delight; but such cases are always to be watched with extreme solicitude. Now is the time to commence stimulants and tonics; if the bowels are tympanitic and tender, give oil terebinth., and administer opiates until refreshing sleep is induced—if bloody evacuations, blister and give astringent enemæ.

After the fever has ceased, and the system is left in a state of prostration, it is necessary to give a considerable quantity of alcoholic stimulants; and it is a little astonishing to see how much a patient can consume, without producing its inebriating effects.

I have seen the *Gelseminum Sempervirens* (yellow jessamine) highly spoken of as a valuable and potent remedy in typhoid fever, but, from its domestic qualities, I consider it an empirical agent; though I will not condemn it until I have had an opportunity of testing its real worth.

I must close my remarks on this fever, though I do it reluctantly. It requires a pen more masterly than mine, an experience of more years than I can boast, an array of remedial agents more sanative than I am able to command, to solve its cause, its pathology, and to write its successful treatment. I commend it to the careful consideration of the profession. Perhaps some fortunate follower of Æsculapius may yet have the enviable pleasure of exclaiming, "Eureka! Eureka!"

During the winter months we had but comparatively few cases of Pneumonia, notwithstanding the unprecedented quantity of rain.

A majority of the cases which came under my observation were complicated with typhoid fever. As a general rule, they yielded readily to treatment, though the termination was lingering and convalescence slow.

The treatment employad consisted principally, in the first stage, of tart. ant. et pot. under which the acute symptoms rapidly succumbed; after which, the usual treatment was pursued, consisting of calomel and ipecac., pushed to slight salivation if required, rubefacients, blisters, expectorants, &c. In the latter stage I have found no remedy so valuable as the comp. tincture of cinchona, combined with comp. syrup scilla. It makes an admirable expectorant and tonic, while it is highly stimulating and anti-periodic.

Surgery.—About the 1st of February, Drs. B. T. Green, Winston, Whitley and myself, amputated the leg of Miss B., a young lady of 23 years of age. Seven years previous to the amputation, she received the contents of a loaded gun in the ankle from which she never recovered. The bones had softened, and were continually discharging pus, and the limb had become greatly atrophied, preventing locomotion. We amputated by the flap operation about three inches below the knee, using æther for our anesthetic. The operation was performed in about five minutes, and no unhappy consequence ensued. A hearty granulation soon set in, and she now has a beautiful stump, which will soon be in a condition to have an artificial leg adjusted. Some minor operations came under my care, which do not present sufficient interest for publication.

Obstetrics.—Under this head there is space for so much to be written, that I scarcely know where or how to commence. It is an appalling fact that there are more diseases, peculiar to woman, which come to the knowledge of the physician, than of all the diseases which "man is heir to"; and what is still more to be pitied, they are on a rapid increase. May we not here inquire into this frightening degeneration of female health?

Look, reader, in your palace of fashion; behold its fairy-like inmates, made in the image of God, and bearing his impress. The hour is late, the night chill and bleak. The hall is rever-

berating with the strains of soul-entrancing music—while these *tender creatures* are paying tribute to the memory of Terpsichore. Exhilerating wine displays itself in the excited laugh, in the flashing eye, and in the sallies of wit. In short, the brain is strung to its utmost degree of excitability, and the body laboring under fatigue. Under such circumstances, how do we find the delicate female attired? Her bosom is dressed with "muslin fashionably thin, arms bare to the shoulder, and feet covered with wafer-soled slippers. Every internal organ is groaning under the compression of stays and laces. Under this state of mental excitement and physical oppression, how is it possible for the delicate tissue of the lungs to perform its healthy action, and maintain its normal elasticity? How can *any* organ perform its allotted duty? In the young, how can the physical organization attain its proper growth and form, or the mental faculties fully develope themselves—for when the one is encroached upon, or its development in the slightest degree suspended, the other is a ready and lasting sympathizer."

Woman cannot censure the physician for her ill-health and weakly constitution—the condemnation rests upon herself. The absurd edicts of the mighty Moloch of fashion, have too overwhelming a sway upon her mind—a fact which should call forth the denunciations of the medical profession. It is our legitimate sphere, as well as our imperative duty, to cry aloud against this growing evil. No true philanthrophist can remain silent and see woman, the source of all our pleasures, committing wholesale suicide without raising a warning voice. Should we remain silent and see thousands of women annually sinking to the grave, we may justly be accused of having interested motives in our silence. Editors deserve much credit for the part they have taken in remedying the evil; but their battery consists principally in ridicule and burlesque, while they do not make the proper discrimination between the fashions that are conducive to health and those that are deleterious. The *hooped skirt*, for instance, has been almost universally denounced and ridiculed by the editorial corps, wherein they have committed a most egregrious error. There is no article of dress which should receive

more patronage from the ladies than the *hoop*. It has superseded the *half-dozen* heavy skirts formerly worn. It combines grace, beauty, and elegance, while it gives ease and comfort to the wearer.

But, lest I may be considered officious, I will desist for the present, promising a few broken doses for the future. I cannot help feeling an interest in the subject, when I know countless diseases of woman are daily originating from pernicious habits of dressing. On the 23d of October, I was called to see Mrs. P. On my arrival, I was informed that she had been in labor for thirty-six hours, and that for the last twelve hours there had been no advancement in the delivery. Her pains had almost ceased, and she was becoming quite weak and much exhausted. On examination, I discovered that the head was lodged in the pelvis, and required rotating. I soon succeeded in turning the vertex to the left ascetabulum, when I commenced administering ergot. Contractions soon came on, and a dead child was expelled. Upon introducing my hand, I found the placenta firmly attached to the uterus, throughout its entire circumference. Here my patient had to submit to the painful and tedious operation of having the placenta dissected from the womb with my fingers, which required nearly an hour. What was remarkable, and fortunate for the woman, no hemorrhage ensued, but for several days the womb was considerably inflamed, which was combatted by the usual anti-phlogistic remedies.

About the middle of April, Dr. Winston and myself were summoned to a negro woman, the property of Mr. M. Like the case above, she had been in labor nearly two days, and had considerable hemorrhage when first taken. Her pains had become quite weak, and there was not sufficient dilatation to ascertain the presentation. We administered ergot and in a few hours the os became sufficiently dilated for us to discover that it was a shoulder presentation. For a long time we endeavored to grasp the feet and turn, but all our efforts were vain. The hand could not be carried beyond the side of the child. Finding it impossible to succeed in delivery, and the pain having ceased, we called to our assistance Dr. W. R. King, of Louisburg, when we ampu-

tated the arm of the fœtus and then eviscerated it. We then made a hook of the spindle of a spinning wheel, and succeeded in fastening it over one thigh, when, upon gentle traction, it was expelled. The child was large, and had been dead so long that putrefaction had taken place. Considerable inflammation followed for several days, but she recovered, and is now in good health.

ON THE QUANTITY OF OXYGEN CONTAINED IN THE VENOUS BLOOD OF GLANDULAR ORGANS, WHEN IN ACTIVITY AND REPOSE, AND ON THE EMPLOYMENT OF THE OXIDE OF CARBON IN DETERMINING THE PROPORTION OF OXYGEN IN THE BLOOD.

BY CLAUDE BERNARD.

Translated from the French, by EDWARD WARREN, M. D.

IN a communication to the Academy, on the 25th of January last, I showed that in the normal or physiological state, the Venous Blood of glands is *rutilant* when the secreted product is expelled, and that it is *black* when these organs expel nothing, and are quiescent.

In another communication, I have pointed out by what means the two orders of nerves control the variations of color which take place in the glandular venous blood. On this occasion I desire to investigate the chemical modifications of the blood which connect themselves with the changes of color in the same vein.

But I must expressly state, that no chemical analysis is attempted in this connection. This examination of glandular venous blood is only undertaken for the purpose of ascertaining the relative quantity of oxygen—that gas to which the red color of the blood has always been ascribed; and I should not have encroached even thus far upon chemical ground, if I were not governed by considerations entirely physiological, as I shall illustrate by the employment of a novel and simple means of determining the quantity of oxygen in the blood.

About ten years since, I made some experiments on the poisoning of animals by the oxide of carbon, which I have since repeated in my course at the College of France in 1853 and

1856. While studying the toxic effect of the oxide of carbon on the blood of living animals, I discovered that this gas rapidly poisoned the animal by substituting itself for the oxygen of the blood-globules, and not yielding its place to the oxygen of the air; from which it resulted, that the blood-globules, being somewhat paralyzed, were incapable of absorbing oxygen, and then circulated as inert bodies, incapable of supporting life.

If all the globules are subjected to the influence of a sufficient quantity of the oxide of carbon to displace the whole of their oxygen, death occurs immediately, and life cannot be restored by artificial respiration; whilst, if a portion of the blood has escaped this deleterious impression, death supervenes more slowly, &c. In a word, I have considered the decided toxic action of the oxide of carbon as depending upon its remarkable affinity for the constituents of the blood-globules. In fact, the oxide of carbon has a greater affinity for the globules of the blood than the oxygen, since the oxide of carbon rapidly displaces the oxygen, whilst the oxygen cannot displace in its turn the oxide of carbon. It is this singular toxic property of the oxide of carbon, the effect of which I was the first to recognize, that induced me very naturally to employ this gas for the purpose of displacing the oxygen of the blood. This method offers, above the old processes, the advantage of being more rapid and certain, because the causes of the disappearance of oxygen during the operation are eliminated by the same toxic action of the oxide of carbon on the blood. For two years I have employed this method in a great number of investigations, and during the last winter, at the College of France, in my course, which was principally devoted to a study of the blood, I have publicly demonstrated the advantages of this plan of analysis,—supporting it by numerous experiments performed by M. Leconte, and which were instituted as a means of determining the relative amount of oxygen in the various organs of the body.

This, in a few words, is my manner of operating: I draw out the blood from the vessels by means of a graduated syringe, and pass it rapidly, by the aid of an iron canula, into a graduated glass tube placed over mercury, and previously filled with the

oxide of carbon gas. I thus obtain the blood, unaffected by the contact of the atmosphere (*Loc. cit.*, p. 166). As soon as the blood is thus introduced, I shake it violently, in order to effect a mixture, and to prevent coagulation. I keep the blood and the oxide in contact for an hour or two, at a temperature of thirty to forty degrees, taking care to agitate the blood, during this interval, two or three different times. The volume of the gas does not change in its totality, because the oxide displaces the oxygen volume for volume. Under the influence of the oxide of carbon, every variety of blood takes a persistent vermilion tint, which I pointed out long since, as characteristic of the oxide of carbon when brought in contact either with blood in the vessels of living animals, or that removed from the body.

I usually employ, for each experiment, twenty-five cubic centimetres of the oxide of carbon for fifteen cubic centimetres of blood. With this quantity of gas, all the oxygen of the blood can be displaced, which may be established by adding more of the oxide, and noticing the fact that, in this second levigation no appreciable amount of oxygen can be discovered.

For the analysis of the gaseous mixture wherein the displaced oxygen is found, the ordinary methods can be employed, as, for instance, the treatment of carbonic acid by potassa, of oxygen by pyrogallic acid, and of the transformation of oxide of carbon, when that substance is used, into carbonic acid, by means of the electric spark.

After this preface, rather long it is true, but I believe necessary, I come to the essential object of this communication, which is, to ascertain whether the *red* glandular venous blood contains as much or more oxygen than the black glandular venous blood? I have thought that it was necessary to put the question thus. In fact, in the actual condition of our knowledge, two hypotheses only are possible as to the cause of the vermilion hue of that blood which flows from a gland, whilst performing its function, with such activity that it is actually animated with pulsations when the secretory action is very intense. It might be supposed that the red venous blood is simply arterial blood, which has traversed the capillaries so rapidly that it has

not had time to become venous, that is to say, to have its oxygen displaced by carbonic acid gas.

But it may be admitted, also, that the red venous blood is ordinary venous blood, with this difference, that it does not remain dark, because, being produced at the moment of secretion, it is robbed, by means of the glandular secretion, of its carbonic acid, by which it would have been rendered dark, all of which really takes place when the gland does not secrete, and the carbonic acid cannot escape.

This last opinion is strengthened by the fact, that all the liquid secretions contain a considerable portion of carbonic acid, either in a state of solution or combination. The relative amount of oxygen contained in the blood when it enters a gland, and at the moment of its exit, is the only sure means of determining between the truth of the two hypotheses: thus, if, at its exit from the gland, the *red* venous blood contains more oxygen than the *dark* venous blood, or as much as pure arterial, it is clear that it is not venous blood; whilst, on the other hand, if the red venous blood affords less oxygen than arterial blood, or the same quantity as the black venous blood, we ought to believe that during the process of secretion, arterial blood becomes venous in the ordinary manner, with this peculiarity, that it remains red, because it parted with its carbonic acid instead of eliminating that gas through the pulmonary organs.

These, then, are the terms of the problem which I propose to solve; and let us see what we are taught in this connection by actual experimentation.

I have operated upon the blood of the renal vein, because the size of that vessel allows us to obtain with facility a sufficient quantity of blood for analysis.

On a vigorous dog, whilst engaged in the process of digestion, I opened the renal vessels of the left side, with requisite care, and drew out rapidly fifteen cubic centimetres of blood, bringing it in contact with twenty-five cubic centimetres of the oxide of carbon, observing at the same time that the urine flowed abundantly from the ureters, and that the venous blood was nearly as red as that of the artery. Immediately afterwards, one of the

multitudinous divisions of the *renal* artery was cut, at its entrance into the kidney, and from its central end I drew out fifteen cubic centrimetres of blood, which I also mixed with a like quantity of the oxide of carbon. Then, in order to disturb the urinary secretion, I removed the fatty capsule of the kidney. The urine ceased flowing by the ureter in a few moments, and the blood of the vein became as dark as that of the vena cava. At this moment, I withdrew fifteen cubic centimetres of the dark *renal* venous blood, which was likewise placed in contact with twenty-five cubic centimetres of the oxide of carbon. After remaining for one hour in a bath, at a temperature of from 30° to 40°, the analysis of the blood previously described, gave the following results for the quantities of oxygen contained by them, calculated for 100 volumes of blood.

	Vol. of Oxygen.
(1) For the red venous blood	17.26.
(2) For the arterial blood	19.46.
(3) For the dark venous blood	6.40.

In a second experiment there was found 16 for the 100 of oxygen in the red venous blood, 17.44 in the aortic arterial, and 6.43 in the blood of the vena cava.

These experiments demonstrate that the red venous blood of the kidney (and it is presumable that the same is true of the other glandular bloods) differs from ordinary blood, in that it is not *deoxydized.* Thus is our original hypothesis verified, in as much as this blood preserves the characteristics of arterial. However, if this is true for the proportions of oxygen discovered therein, the proposition is not absolutely exact. In fact, the red glandular venous blood contains much less fibrine than the arterial; there is less water in it, because that of the secretions has been substracted; and also, it is more susceptible of alteration (decomposition), that is to say, it becomes spontaneously black much more rapidly when exposed to the atmosphere.

However this may be, in observing for the moment the veritable object of our research, *i. e.*, what concerns the proportion of oxygen in glandular venous blood, we perceive this remarkable fact—it is whilst performing their functions at the moment of secretion, that the glands permit the blood to circulate

without deoxydyzing it, and it is whilst they are not acting, and when no product is expelled, that the blood which escapes is dark, deprived in a great measure of its oxygen, and charged with carbonic acid.

Here again appears the antagonism between the glandular and muscular systems, to which I have often referred. In the muscles, the blackness and deoxydation of the blood passing through them are in direct proportion to the energy of their contractions. In the glands the blood is more red, and less deoxydized, in direct proportion to the activity with which the secretory function is performed. Should we not regard this antagonism in the phenomena thus made apparent, as indicating a *radical* difference both in the modes of nutrition, and the functions of glands and muscles? In a word, may we not affirm that the muscles consume oxygen in direct ratio to their activity of function — as is directly opposite with glands? Or should we rather, in view of this singular phenomenon, conceive doubts respecting the truth of our conclusions in regard to the functional states of the glands?

I incline to the latter opinion; and I think that these researches will lead to another interpretation of that which has heretofore been esteemed the states of activity and of repose of glandular organs, and will induce us to distinguish a state of chemical activity, and another purely mechanical. I could already adduce several arguments in support of this opinion; but I will conclude with the simple facts above enumerated, limiting myself to a simple enunciation of this obscure proposition, which will serve as a point of departure for future explorations.

COUNTER-POISONING.—An exchange says, that a man who was bitten by a copper-head snake, in Rockbridge County, Virginia, died in twenty-four hours. A quart of whisky was administered to him, and the attending physicians attributed his death to the *remedy*.

INTERESTING LETTER FROM THE DISTINGUISHED VETERAN, DR. SAMUEL A. CARTWRIGHT, OF NEW ORLEANS,

ON THE SOUTHERN FORM OF PNEUMONIA, AND THE HISTORY OF THE INTRODUCTION OF LARGE DOSES OF QUININE IN FEVER.

[*Published by request of the Granville (N.C.) Medical Society.*]*

NEW ORLEANS, MAY 7TH, 1859.

Dr. MANSON.

Dear Sir:—Yours of the 25th ult. has just come to hand, but not the Virginia Medical Journal containing your article on Pneumonia, which I have not had the pleasure of seeing or reading. It is very flattering to an author to have his writings favorably noticed, which have been recently published, but it is doubly so to have them favorably noticed by a new generation, after a third of a century has passed away. My Monograph on Pneumonia Biliosa, published in the Medical Recorder of Philadelphia, bears on its face that it was written in April, 1826, very nearly a third of a century ago. You are pleased to say that you "think it contains the best description of the cold stage in pneumonia which has ever been written," but you desire me to inform you more particularly "whether the cold stage in this disease, as you (I) observed it, returned periodically or not?" At page 65† you will see it stated that a "remission of the symptoms generally happens in the morning, or in some other part of the twenty-four hours: it is more favorable when it occurs in the morning." In Natchez, as in New Orleans, Philadelphia, and, I believe, in all of our cities, the remittent fevers and pneumonias are not so decidedly paroxysmal as in the vicinity of water-courses, marshes, &c., at some distance from the blocks of the cities. The pneumonia in the vicinity of Natchez partook of the type of the prevailing intermittent and remittent fevers. In the city, the periodical cold stage was less evident, or scarcely perceptible, amounting to nothing more than a coldness of the tips of the fingers, ears and toes, as you describe. You were pleased to ask me "who first used large doses of quinine in the febrile exacerbation in the South"? Put *world* for *South*, and I answer, that the records of Medicine do not furnish any example of quinine having been used in large doses anything like as far back as April, 1826, when I wrote the

* The President (Dr. O. F. Manson) read a lengthy, interesting, and very able letter from the distinguished Dr. Cartwright, of New Orleans, on the treatment of Pneumonia, and giving a minute and circumstantial history of the introduction of Quinine as a sedative febrifuge in Fever, which, on motion of Dr. P. P. Peace, he was requested to submit for publication in the North Carolina Medical Journal.

Extract from the Minutes of the Granville Medical Society.

† Medical Recorder, vol. x. Philadelphia, 1826.

article on Pneumonia Biliosa, strongly recommending the sulphate of quinine in large doses in that disease, with the single exception of a paper published by Dr. Perrine near about the same time, in Chapman's Journal. I may here observe, that Dr. Perrine was a *protégé* of mine, who came from Illinois to Natchez. Dr. James A. McPheeters and myself indoctrinated him into the mysteries of treating fevers and pneumonia with quinine. He settled in the country about twelve miles from Natchez, where the quinine practice in his hands was so successful, that Dr. James Metcalf, in the same neighborhood, was compelled, in self-defence, to adopt it to retain his business. Dr. James Metcalf had a brother a student of Medicine in Paris at the time, to whom he communicated the wonderful virtues of the sulphate of quinine in six-grain doses during the paroxysm of certain fevers, attended with very frequent pulse and great determination of blood to the head, and also as very valuable in Pneumonia Biliosa. It was through that brother, Dr. Volney Metcalf, that the French were induced to try the article in large doses. It was more than ten years after the introduction of the quinine practice in certain forms of remittent fever and pneumonia, before the physicians of New Orleans, or anywhere else, adopted the practice, with the exception of Dr. Thomas Fearn, of Huntsville, Ala., who began its use in 1833 or 1834. For a number of years the quinine practice was confined almost entirely to Drs. McPheeters, Perrine, Metcalf and myself, and a few young men who learned the practice from us, and adopted it in order to succeed in business. Dr. Bouldin was one of these. Not succeeding in Natchez, he settled in Claiborne county, Mississippi, where his great success with the quinine introduced him to a large and lucrative practice. He mixed prussiate of iron with it—his usual dose being six grains of each. In scorbutic and hemorrhagic conditions of the system, the prussiate of iron was found to be a valuable adjuvant.

Subsequent experience has convinced me that six grains do better than larger or smaller doses, and that the remedy should never be pushed after it begins to produce its peculiar effects upon the ears. Patients were found to bear it better when given in combination with morphine, Dover's powder, aconite, valerian, extract canabis indica, &c.,—one or more of these articles with warm drinks to determine to the surface, and in pneumonia, with some expectorant. We preferred giving the quinine in solution to pill.

* * * * * * * * * * * * *

North Carolina claims the credit of being the first to declare her political independence. I would be glad to fire you with the ambition to make her take the lead in freeing the South from its worse than colonial vassalage to European and Northern Medical authority. Even

in our Southern Medical schools, a candidate for graduation would stand a poor chance of getting a diploma if he did not know the exact particulars of the fashionable mode of treating diseases in the European hospitals, among the half-starved inmates of those institutions. If want of meat and bread, want of fuel to make fires, clothing and house-room, and the comforts of life, were as prolific in causing diseases in the United States as in Europe, and in some of our Northern cities, then there would be some little more show of reason in looking to Europe and the Northern cities for rules of practice in treating the diseases met with on the Mississippi of the West and the Tar and other rivers of the East. Our negro population are unknown in Europe, and are represented in medical books, as they are erroneously supposed to be, as white men with black skins. Our diseases are almost entirely unknown as our negroes are. They are imagined to be some variety of types observed in the North, instead of having direct types of their own. I am glad that North Carolina, so slow to move, is waking up to the fact that our Medical journals, even the old ones, containing the experience of our Southern diseases, are of more value than what the the auscultators in the London and Paris *hear* unless they had our diseases to listen to.

Very respectfully, your obedient servant,

SAMUEL A. CARTWRIGHT.

N. B.—Since writing the above, your "Sketches of the Endemic Diseases of the Roanoke Valley of Virginia and North Carolina," your Report to the North Carolina Medical Society on "Malarial Pneumonia," and a paper on the same disease published in the last February number of the Virginia Medical Journal, have been carefully read by me. *My opinion is that they ought to be extensively published, and that no Medical student should receive the degree of Doctor of Medicine in any of our Medical Schools who cannot stand a rigid examination upon them.* The high price of negroes is making a salutary reform in Medicine. In the Southern States it has already driven the servile submissionists to European medical authority out of the country upon the cities. They cannot succeed in the country, because they cannot cure pneumonia and remittent fevers. In the cities they can make a living by doing a little surgery, editing medical journals, re-publishing European books with stupid notes and commentaries, and getting professorships in *European* medical schools, *located* in *America.* The great majority of our city practitioners are either learned servile submissionists to European medical authority, or arrant quacks, either of whom would starve in the country, because they cannot cure the diseases of negroes. A negro is too valuable to trust to a quack when

afflicted with any serious disease, or to a learned submissionist who pins his faith to that European authority which ignores the existence of any such people as negroes; stoutly contending that they are to be cured by the same remedies, when sick, as those which are found to be the most successful in the treatment of the miserable paupers in the hospitals of London, Edinburgh, Dublin and Paris.

Yours respectfully,

SAMUEL A. CARTWRIGHT.

It is a source of pride and pleasure to the undersigned to have it in his power to present the above letter from a gentleman who has even distinction, not only among his brethren at the South, for his useful contributions to practical medicine, but who has obtained honorable recognition from Northern umpires.

To those unacquainted with Dr. Cartwright, I may state, that, in his younger days, he attained considerable reputation for essays published in this country. In my researches among these neglected and forgotten works, I find it noticed that he received two gold medals from Harvard University, being the Boylston Premiums for dissertations; on another occasion a silver pitcher was awarded to him by the Medico-Chirurgical Faculty of Maryland, besides other prizes in Philadelphia. Later, however, the Doctor received a present (which he doubtless values most highly) of a magnificent gold vase, valued at above $1000, from the citizens of a settlement near Natchez, for his success in the treatment of an epidemic in 1833. The Doctor's highest claims to distinction, however, rest, I conceive, for the conspicuous part which he has enacted in the introduction of Quinine as a sedative in fever, &c.,—*the great medical fact of the nineteenth century.*

O. F. MANSON.

☞ The cholera in Germany, the Lancet says, is still increasing. In some villages half the inhabitants have been carried off. Agricultural operations have been suspended, and the cattle let loose in the fields, there being no one to attend them.

Bibliographical Notices.

On Poisons in relation to Medical Jurisprudence and Medicine. *By* Alfred Swaine Taylor, *M.D., F.R.S., &c. Second American, from the second and revised London edition.* Philadelphia: Blanchard & Lee. 1859.

As a complete and reliable work on this important subject, this excellent book of Dr. Taylor's ranks second to none, and well deserves the attentive study of every practitioner of Medicine. The frequency with which physicians are called upon for scientific opinions in cases of poisoning, renders it particularly important that each one of them should have at command a work wherein the whole subject of toxicology is treated of, in a manner commensurate with its merits, and at the same time concise, accurate, and complete. To those who appreciate the necessity for such knowledge, we beg leave to recommend this book, assured that, in so doing, we are conferring a benefit on the profession, and contributing towards the advancement of humanity as well as of science.

Anatomy, Descriptive and Surgical. *By* Henry Gray, *Lecturer on Anatomy, St. George's Hospital. First American edition.* Philadelphia: Blanchard & Lee. 1859.

This is by all comparison the most excellent work on Anatomy extant. It is just the thing that has been long desired by the profession. It meets, and fully supplies the want which every student of anatomy has felt since the days of Hippocrates. It is a complete, thorough, and accurate exposition of all that relates to this most interesting subject, both in its "Descriptive" and "Surgical" aspect. And, indeed, whether we consider the typographical excellencies of the work,—the beauty and accuracy of its illustrations, or the truth and clearness of its descriptions, there can be no hesitation in pronouncing it one of the bibliographical successes of the times. With such a guide as this, the student of anatomy, the practitioner of Medicine, and the surgical devotee, have all a newer, clearer, and more radiant light thrown upon the intricacies and mysteries of this wonder-

ful science, and are thus enabled to accomplish results which hitherto seemed possible only to the specialist. The plates, which are copied from recent dissections, are so well executed, that the most superficial observer cannot fail to perceive the positions, relations and destinctive features of the various parts, and to take in more of anatomy at a glance, than by many long hours of diligent study over the most erudite treatise, or, perhaps, at the dissecting table itself. We take pleasure in recommending this book to our readers.

WOMAN AND HER DISEASES. *A Series of Letters to his Class, by Professor* CHARLES D. MEIGS, *of the Jefferson School, Philadelphia.* Blanchard & Lee. 1859.

Doctor Meigs is the "Domine Sampson" of the profession. He is brimful of pedantry, exaggeration, and pomposity, but possesses, at the same time, a simplicity, straightforwardness, independence and erudition, which invest with peculiar interest all that emanates from him. It is true that the Professor has the advantage of the Pedagogue on the score of *common sense*—which, by the way, is the most *uncommon* sense after all,—but still the parallelism between the two is remarkable. Who knows, so well as the learned Doctor, how to clothe the plainest common-places in the most gaudy, gairish and superfluous vestments?—who raises the most furious of tempests only to "waft a feather or to drown a fly"?—who shoots at sparrows with artillery heavy enough to shake a city or to sink a squadron? and yet, who can discourse more eloquently and learnedly of masters, authors, principles, and all that relates to the subject in which he is interested? Who can bring richer, nobler, or more grateful offerings to the shrine of Science than this faithful, but fanatical student of Nature? Whose tropes are more apt, fascinating and instructive than his, though they do savor of the grandiloquent and bombastic? Whose style is more charming, more striking, more eloquent than Dr. Meigs', whether it be the very essence of affectation, hyperbole and exaggeration, or not? There can be but one answer to these questions from every candid mind, and we have risen from the reperusal of this excellent

work all the more convinced that its author is a scholar, a philosopher and a gentleman; and still better prepared to accord to him the honor he has so justly merited.

URINARY DEPOSITS, THEIR DIAGNOSIS, &c. *By* GOLDING BIRD, *M.D., F.R.S.* Blanchard & Lee. 1859.

The untimely fate of Dr. Bird invests with peculiar interest this admirable contribution to medical science; but independent of all adventitious circumstances, it has sufficient merit of its own to render it *classic*, and to place it in the front rank of the professional literature of the day. The publishers have shown their usual taste and skill in the getting-up of this edition.

MINUTES OF THE TENTH ANNUAL MEETING OF THE MEDICAL SOCIETY OF THE STATE OF NORTH CAROLINA, *held at Statesville, N.C., May,* 1859. Fulton & Price: Wilmington, N. C.

This is a neatly printed pamphlet of 27 pages, giving an account of the late meeting of the State Society, together with an address from the retiring President, Dr. McKee, and the constitution of the Medical Society of the State of North Carolina.

We have previously stated, that this meeting was a decided *success*, and as an evidence of it, we beg leave to refer to the following facts. Upon calling the roll, the following members were found present.

OFFICERS:

DR. WILLIAM H. McKEE, President.
" JAMES G. RAMSEY, Vice-President.
" PETER E. HINES. "
" JOHN R. MERCER, "
" WILLIAM GEO. THOMAS, Cor. and Recording Secretary.
" C. W. GRAHAM, Treasurer.
" J. J. SUMMERELL, Orator.

MEMBERS:

DR. N. G. PITTMAN,	DR. THOMAS H. AVERA,
" CHARLES E. JOHNSON,	" E. R. GIBSON,
" S. S. SATCHWELL,	" GEORGE H. COKE,
" P. T. HENRY,	" R. H. WINBORNE,
" CHARLES F. DEWEY,	" DAVID COX,
" E. BURKE HAYWOOD,	" GEORGE W. HODGES,
" JAMES F. McREE, JR.,	" J. M. HAPPOLDT,
" A. B. PIERCE,	" JAMES R. HALL,

DR. HENRY W. FAISON,
" OWEN HADLEY,
" C. T. MURPHY,
" J. S. LEACH,
" OTIS F. MANSON,
" HUGH KELLY,
" M. M. MURRAY,
" F. M. HENDERSON,
" A. M. NESBITT,
" M. WHITEHEAD,
" ROBERT F. HACKETT,
DR. GEORGE A. FOOTE,
" A. S. WIGGANS,
" PETER CUSTIS,
" A. S. COTTEN,
" W. R. SHARPE,
" B. F. GRAHAM,
" ALEX. TAYLOR,
" SAMUEL J. IREDELL,
" W. J. BUSBEE,
" J. C. MARRIOTT.

And, on motion of Dr. Whitehead, the following gentlemen were elected members of the society:

DR. SAMUEL MASTERS,
" EUGENE GRISSOM,
" R. L. PAYNE,
" WILLIAM H. HALL,
" R. N. CHAMBERS,
" J. RHODES TAYLOE,
" T. P. COLEMAN,
" A. A. LAWRENCE,
" WYATT M. BROWN,
" J. G. THOMAS,
" D. A. TAYLOE,
" F. M. ROUNTREE,
" WILLIAM H. H. CONNER,
" JAMES HICKERSON,
" E. B. HAMPTON,
" T. W. JOHNSTON,
" A. TORRENCE,
" J. W. CALDWELL,
" R. L. BEALL,
DR. CYRUS A. ALEXANDER,
" L. S. BINGHAM,
" RICHARD R. BARR,
" C. HAPPOLDT,
" W. A. COLLETT,
" THOMAS WILLIAMS,
" JAMES K. NESBITT,
" EDMUND F. ASHE,
" JAMES W. HALL,
" R. T. CAMPBELL,
" R. B. RIDDICK,
" WILLIAM M. RIDDICK,
" N. A. LOCKE,
" D. B. WOOD,
" O. P. HOUSTON,
" W. M. CAMPBELL,
" JAMES HAY,
" JAMES McGUIR.

Making in all eighty-one members present at the meeting. This is the largest number that ever assembled on such an occasion, and we doubt whether the records of any State Society can show so large an attendance as this. The truth is, the Profession in North Carolina is thoroughly awakened to the importance of a combined and harmonious action, as a means of securing those reforms which its best interests so imperatively demand, and whose beneficial results are yet to be recorded in letters of gold, upon the history of the State.

We are pleased to find, that a motion offered by Dr. Hines of Raleigh—"that after the adjournment of this meeting of the Society, there shall not be appointed a committee on nominations,"—was adopted; and that, subsequently, it was determined to place all appointments immediately in the hands of the Society. This is certainly the fairest and most democratic method, and as it strikes a death-blow at all combinations, cliques, and wire-pulling, it effectually removes an obstacle upon which more organizations of a similar character have been wrecked than upon all other things combined.

Drs. Cyrus A. Alexander and Otis F. Manson, prompted by a most laudable desire to increase the usefulness of the Society, very generously offered prizes for the competition of the members of the association. Dr. Alexander offers a prize of fifty dollars for the best essay "On the use of Tobacco, and its injurious effects upon the system." Dr. Manson will present a *gold medal* for the best essay "on any Medical subject."

We have no doubt these prizes will call out the best talent of the profession, and that something will be produced highly creditable to the State.

The Committee on Finance reported the treasury in a very healthy and vigorous condition.

The Committee on the Medical Journal reported that work in a prosperous state, and recommended it to the especial patronage of the Society. Dr. Satchwell offered, as a sort of amendment to this report, "that the Editor be instructed to insist upon a system of advanced payments,"—which he (the Editor) shall take great pleasure in doing for the future.

Dr. Manson very kindly offered the following resolution,—which, we are proud to state, was adopted without a dissenting voice.

"*Resolved*, That the thanks of the Society be, and are hereby tendered to Dr. Edward Warren, for the able and satisfactory manner in which he has conducted the North Carolina Medical Journal."

We have already expressed our grateful sense of the compliment thus paid us by the Society, and will only say that, for the future, we will labor to deserve its good opinion.

Dr. N. J. Pittman announced the death of Drs. Battle, Speller and Faison, and offered the following preamble and resolutions, which were adopted:

"WHEREAS, since our last annual meeting, it has pleased Divine Providence to remove by death three of our worthy members and co-operators, Dr. Joel D. Battle, of Wayne; William H. Speller, of Bertie, and Elias K. Faison, of Sampson:

"*Resolved*, That whilst we submit with becoming reverence and humility to the decrees of Providence, we can but deplore the great loss to this Society, and the respective families and communities of the deceased.

"*Resolved*, That we tender to the families of the deceased our heartfelt sympathies in their sad bereavement.

"*Resolved*, That a copy of this preamble and resolutions be forwarded by the Secretary to the respective families of the deceased, and that they be embodied in the minutes of this meeting of the Society."

Dr. James F. McRee, Jr., announced the death of Dr. A. J. DeRossett, Sr., of Wilmington, an honorary member of this Society, and presented the following biographical sketch and resolutions, which were adopted:

"Dr. Armand John DeRossett, an honorary member of this Society, died in Wilmington on the first day of April, 1859, in the ninety-second year of his age.

"His very advanced age, his high professional attainment, his extensive and successful practice, and his devotion to the Medical Profession as student and practitioner, for a period of more than seventy years, entitle him to a biographical notice among the proceedings of our Society.

"His paternal ancestors were from Narbonne, France, in the neighborhood of Montpelier, and were of so-called noble blood. "I mention this," says the Doctor himself, in a paper which he addressed to his children at the advanced age of eighty years, "not as entitling us to any consideration or respect other than such as our conduct and character, as men and christians endeavoring conscientiously to perform our duty to God and our fellow-men, may justly claim." They were Protestants. They were forced to emigrate because of their religious opinions, and the family titles and estates were forfeited because of their emigration. Thus they were compelled to bear their full share of the burdens which were imposed upon the early Protestants, and they were sustained in their losses and governed in their conduct by those high principles which, at a subsequent period, led to the independence of the American colonies, and the formation of the American government. About the period of our revolution the restoration of the family estate and titles was tendered to an uncle of the Doctor's upon the condition that he would return to the bosom of the Romish Church, but the offer was unhesitatingly rejected.

"His great grandfather, Louis, was a military officer, holding his commission from the British government. This is accounted for from the fact

that he was a Huguenot, and from the further fact that the English nation claimed the sovereignty of several provinces of France. He held also the honorable title of "Doctor of Laws," as well as "Doctor of Medicine."

"His grandfather, Armand, graduated in 1720, in Bazle, Switzerland, and was compelled to emigrate by religious persecution.

"His father, Moses John, was born in London, soon after the emigration, and the whole family subsequently came to this country. Here his father married "Amy Ivey," whose sister was married to General Moore, of revolutionary memory.

"The Doctor was born in New Hanover county, on the 17th day of November, 1767. He was a graduate of Princeton College, studied medicine under the celebrated Dr. Rush, and received his medical diploma from the College at Philadelphia. At his graduation he received the highest mark of distinction which was then conferred by the College. His Thesis was written in Latin, and published by the Faculty.

"He married a grand-niece of David Hume, the historian, and settled in Wilmington for the practice of his profession. His grandfather, his father, himself and his two sons, have all been practitioners of Medicine in that place. In 1847 the Doctor wrote of himself—"I have been practising medicine fifty-eight years, and have prescribed for six generations in one family." At that period he was eighty years of age. His last professional service, however, was rendered at the age of ninety-one, in the capacity of accoucheur to a lady. He must have practised medicine, therefore, for a period of sixty-nine years. Such another instance can scarcely be found in our country.

"This brief statement bears ample testimony to the estimation in which he was held as a physician. For sixty-nine years he retained the practice which he had acquired in early life. His character was formed amid the stirring events of the revolution. He was possessed of great energy, a sound judgment, and strong sense. His distinguished scientific attainments, therefore, were applied to the practical duties of his profession in a manner which always commanded a very extensive practice, and this practice was lucrative, and generally successful.

"As a man he was remarkable for his strict integrity. His principles were those taught and practised by his ancestors. His manners were those of the old school, which, we lament to say, are passing away. He was generous, charitable, hospitable, kind and gentle, yet always firm. He was, in every respect, a model of the christian and gentleman."

In conclusion, Mr. President, I move the adoption of the following resolutions:

"*Resolved*, That in the exalted character of the deceased in all the relations of life—in his long and ardent attachment to the profession of medicine, he has left us, individually, a bright example for our imitation; and this Society, as one of its oldest and most esteemed honorary members, a memory venerable for its years, and full of honor.

"*Resolved*, That we offer his surviving family our most sincere sympathy and condolence.

"*Resolved*, That this biographical sketch, and these resolutions, be incorporated in the minutes of this meeting of the Society; and that the Secretary be requested to transmit a copy of the same to the family of the deceased."

We had the pleasure of a personal acquaintance with these most excellent gentlemen, and whilst we mingle our tears with those of their bereaved brethren, can only say, in the words of the poet—

"None knew them but to love them;
None named them but to praise."

On Wednesday, at seven o'clock, P. M., the Society met in the Presbyterian Church, to hear the oration of Dr. J. J. Summerell, of Salisbury, who delivered "an able and highly entertaining address," "on some of the qualifications which should characterize the members of the medical profession."

"This "address," says the Minutes, "was replete with sound sense, and wholesome views of the subject, eloquently maintaining that a holy life and christian character should be to the medical gentleman next in importance to a thorough knowledge of the science and art of Medicine." The thanks of the Society were duly returned to the eloquent gentleman, and a copy of his address requested for the Journal. We are pained to state, however, that the Doctor, for reasons of a private nature, has determined not to furnish us with a copy, and that this most excellent production will thus be lost to the Profession. This is to us a personal as well as a public misfortune, for we were particularly anxious to adorn our pages with the address of our distinguished and much respected friend.

The subject of the Medical Bill was early brought before the Society, and after some discussion upon its merits, a Board of Medical Examiners was duly elected under its provisions.

This was the most important measure of the session; and we hail the action of the Society with peculiar satisfaction, regarding it not as the harbinger of a brighter day for the Profession in the State, but as the actual break of that glorious day itself.

Dr. Manson, nothing daunted by the long guns which our friend Dr. Howard has been bringing to bear upon him for some months, read a synopsis of his replication, and announced his

determination to reply at length through the Journal. We can only play the part of a neutral, offering a fair field and an equal fight to both parties, and ready to present our condolence or congratulations according to the circumstances of the case. There has been already a vast deal of erudition and ability displayed in this controversy, and we trust it will engender no unkindness or ill-feeling.

An ethical case presented itself, which was disposed of in the most summary and satisfactory manner, thus:

Dr. Satchwell, Chairman of Committee on Medical Ethics, presented the following report, which was adopted:—

"*To the Medical Society of the State of North Carolina.*

"The Committee on Medical Ethics, appointed more especially to report upon that species of quackery denominated 'under-charging,' and upon that sort of empiricism referred to in sec. iii, art, 1st, of the Code of Ethics, respectfully report as follows:

"We regret to state to the Society that evidence of a satisfactory character has come to our knowledge of a public offence, grossly reprehensible, injurious to the Society, and inconsistent with professional rights and obligations on the part of one of our members, Dr. Warburton Hill, of Goldsboro. It seems, from the testimony before us, that he has issued, or caused to be issued, to the public, a large placard or medical handbill, a copy of which is in our possession, from which we make the following extract:

"'I expect to get one of the best assortments of medicines and surgical instruments that has ever been offered in this State. ☞ I have also an electrical machine—not a little galvanic battery, but a new machine, that has never been used in the State (except by Dr. Thomas Hill and myself, in Duplin), which I keep at my house, where I am prepared to take all persons afflicted with nervous or chronic diseases of any kind. I will treat such persons medically, and board them at my house (formerly occupied by Sheriff Coor) at the lowest rates.'

"This conduct is sufficient, in the opinion of your Committee, to justify the expulsion of Dr. Warburton Hill from this Society; and we recommend that the Secretary notify him to appear at the next annual meeting of this Society, and show cause why this should not be done.

"In relation to under-charging, embracing such an unworthy and empirical course of conduct as may be instanced where physicians in any town, village or neighborhood refuse to unite with their brother practitioners in adopting, according to their judgment, a reasonable and uniform tariff of charges; or who become so regardless of professional honor as to make it known directly or indirectly, publicly or privately, that he or they will practice *cheaper* than their rival practitioners in the same field: we believe every honorable physician should denounce all such quackery, as inconsistent with chap. ii, art. 1st, sec. 3d of our Code of Ethics. We cannot too strongly recommend this important part of our rules and regulations by the members of our Society, and by all physicians who are really with us in

form as well as in spirit—in action as well as in feeling—and who desire and expect to merit and receive the confidence and fellowship of those who adhere to this provision in our Code of Ethics.

"Respectfully submitted. "S. S. SATCHWELL, *Chairman.*"

Dr. McKee, the retiring president, delivered a very interesting address on the origin and progress of the Society, which was ordered to be printed with the Minutes.

The following appointments were then made by the Society:

President:

DR. N. J. PITTMAN.

Vice-Presidents:

DR. P. T. HENRY, DR. M. WHITEHEAD,
" R. H. WINBORNE, " T. S. LEACH.

Corresponding and Recording Secretary:

DR. WILL. GEO. THOMAS.

Treasurer:

DR. C. W. GRAHAM.

Orator:

DR. W. T. HOWARD.

Delegates to Convention for Revising Pharmacopœia of United States:

DR. W. G. THOMAS, DR. P. E. HINES.
DR. E. WARREN.

Medical Board:

DR. J. H. DICKSON, DR. O. F. MANSON,
" C. E. JOHNSON, " W. H. McKEE,
" C. WINSLOW, " C. HAPPOLDT,
DR. J. G. TULL.

Delegates to National Medical Association:

DR. J. H. DICKSON, DR. B. W. MABRY,
" E. B. HAYWOOD, " J. A. GIBSON,
" J. F. McREE, " M. WHITEHEAD,
" A. B. PIERCE, " J. G. RAMSAY,
" H. W. FAISON, " R. H. WINBORNE,
" O. HADLEY, " A. S. COTTON,
DR. P. T. HENRY.

On motion of Dr. D. A. Tayloe, the Society adjourned, to meet in the town of Washington at 4 P.M. on the fourth Wednesday in April.

It will thus be perceived that the State Society of North Carolina is instinct with life and vigor,—that its prosperity is without a parallel in the history of the State, and that it is nobly fulfilling that exalted and most honorable mission for which it was originally instituted.

SELECTIONS.

Practice.

ON THE VALUE OF QUININE AS A THERAPEUTIC AGENT.

BY MONTROSE A. PALLEN, M. D., OF ST. LOUIS.

THERE is a tendency in the human mind to ascribe causation to mere antecedents — to presume that certain things bear the relation of cause and effect, when the true relation is antecedence and sequence only. From this common error medical science is by no means free. Disease departs from the human system, and the cure is ascribed to the remedies employed, when the true solution lies in the efforts of Nature. This is manifest enough when we reflect on the number of patients recovering under the ridiculously infinitessimal doses of the disciples of Homœpathic practice.

This truth should always be present to the mind of the scientific physician. It inculcates on him the necessity of watching the operations of Nature, and of determining how much she is competent to cast off the elements of disease. Knowing the extent of her powers, we are prepared to assist her when she falters, and even, if necessary, to strengthen her well-directed endeavors.

There is also a tendency in the human mind to grasp with avidity anything positive. Sciences which lead to certain results are always pursued with energy. Hence the charm which the study of mathematics possesses for many persons. Hence the desire of all physicians to master the subject of auscultation and percussion in the exploration of visceral disease. Hence the study of the microscope, and the use of the speculum. In a word, to this desire for certainty we ascribe the anxiety of the physician to listen for sounds which convey correct information, and to inspect diseased organs for ocular demonstration. So, too, in therapeutics, he seizes on a remedy which leads to known curative results; for this reason, quinine, with its well-earned reputation in intermittent diseases, becomes a powerful and popular remedy for good and for evil. The physician, knowing its

virtues, wishes to enlarge its sphere of action; he administers it in various affections; the patient recovers, and that which is a mere sequence, becomes in his mind an effect.

We lay down these propositions as the result of our observations:

A. Quinine is an efficient remedy in intermittent diseases.

B. Many of these diseases require previous medication before it is proper to give quinine.

C. In continued diseases, whether they be fevers or inflammatory affections, quinine is of no value, except at their close, when a tonic is desirable, and then it is no better than many other vegetable bitters.

We need not insist on the value of quinine in intermittent and remittent fevers, and in intermittent neuralgia. In many such diseases nothing else is required; but it also happens, that when the large, flabby and coated tongue denotes that there are elements in the system which ought to be thrown off, it is necessary that free purgation should first be resorted to, the condition of the tongue and the character of the alvine dejections affording the means of judging how far this is to be carried before quinine ought to be administered.

Quinine has its value in intermittent dysentery. In the Fall season, when dysentery has both an inflammatory and a paludal element in its pathology, distinct intermissions become apparent. In this form of disease, the liver is much involved, and the indications are to restore the normal healthy actions of this organ, to subdue the colitis, and to break up the exacerbations. Quinine alone is not apt to succeed—calomel and opium are slow of success, but a combination of each is attended with the happiest results. We do not mean that it is always proper to give them combined; it is better, first, to relieve the inflammatory element with calomel and opium, and then to give quinine alone, or with opium if the latter be required to restrain the irritability of the bowels. Quinine has its value in intermittent pneumonia, an affection by no means of rare occurrence in this locality, and to be distinguished from ordinary pneumonia by the following characteristics: The rigor which ushers it in is more violent and

longer continued, and is followed by intense heat, which, in a few hours, is succeeded by abundant perspiration. During this period there is great headache and sometimes intense lumbar pain; the pulse is rapid and soft, and not full and strong as in ordinary pneumonia.

Dr. Constant, practising in the paludal districts of the department of Lôt, in France, says of this disease, that "there is never any purulent expectoration, these pneumonias never passing beyond the second stage, *i. e.*, red hepatization, the pulmonary engorgement being rather a sanguineous congestion than inflammation. Auscultation and percussion being of the highest value, often revealing the disease when unsuspected, a distinctive feature is the rapid passage from the first to the second stage of the disease, so that eight or ten hours after auscultation had revealed only a circumscribed *râle*, a whole side will be found hepatized. Under the influence of large doses of quinia this rapidly disappears, giving way to returning subcrepitant *râle* during the remission of the fever, but returning again during the paroxysm, if this have not been cut short. The crepitant *râle* of the first stage is almost always moist, the *parchment-cracking râle* only having been heard for a short period, two or three times in more than sixty cases. It invades large surfaces rapidly, being heard posteriorly, sometimes laterally, but never in front. This form of pneumonia especially affects the posterior parts of the lower lobes. It especially appears in summer and autumn, while ordinary pneumonia prevails in spring and winter. It attacks all ages indiscriminately, except early infancy. The blood which flows from a vein is often below the normal temperature, very black and deficient in plasticity. After rest, its surface acquires a bluish color, especially if the patient is taking quinia. The clot is slow of formation and soft. The buffy coat is absent or very thin, and inclines to a bluish color. The condition of the blood, conjoined with the soft pulse and red hepatization, constitutes the chief distinctive sign of the affection.

"In this district, during winter, purely inflammatory pneumonia is met with; but in proportion to the high temperature and the production of malarial emanations, this inflammatory element

is replaced by the paludal one. There are, indeed, three forms met with — 1st, Simple pneumonia; 2d, Spring inflammatory pneumonia, complicated with intermittent paroxysm; 3d, Summer and autumn intermittent pneumonia. The first requires bleeding and antimony; the second, antiphlogistic treatment with quinia, given either simultaneously or subsequently; and the third, quinia in combination with external revulsives. These forms may still undergo further admixture, according as the inflammatory or paludal element prevails, requiring appropriate modification in the treatment."

In dysenteries of a continued type, that is, when they are merely inflammations, quinine is useless. In pure inflammatory pneumonia, no man in his senses ought to prescribe it. If, then, quinine be valuable in these affections when they are of an intermittent type, that is to say, when they have a paludal element mixed up with them, and it be pernicious when they are purely inflammatory and continuous, would not analogy teach us that the same result would follow in fevers? Paludal fevers are remittent and intermittent. Is such the case with typhus and typhoid fevers? We must not confound, as a slight abatement of the frequency of the pulse, a distinct remission. This is observed in many affections after the repose and quietude of night. If, however, we exclude the argument by analogy, experience will teach us. We have seen many cases of typhoid and typhus in our hospitals, in which the quinine treatment has been totally discarded, the rates of mortality decreasing in a greater proportion than they increased under its treatment. According to Dr. E. F. Smith, of this city, who has had ample opportunities to investigate these forms of fever both at home and abroad, the results of his experience are, that in the pure forms of typhus and typhoid, there being no paludal element, the administration of quinine is never attended with beneficial results. Typhoid fever, whose characteristic lesson is ulceration of the bowels, seems to us, from statistics carefully gathered to be made worse by quinine, as the diarrhœa was universally made worse, the head symptoms increased, and the patient more prostrated after the administration than before. In this connection we will

quote the words of Dr. Barclay, in the London Medical Gazette and Times, January, 1853. After giving the reports of the cases treated by him at St. George's Hospital, some with quinine, and some without quinine, he says:

"I must here distinctly state, that when I commenced this report I had no idea what the result would be, and, so far from believing it unfavorable, had hoped, that, excluding some unfortunate cases, the treatment of fever with quinia would prove rather more speedy, safe and effectual, than by ordinary modes. I am sorry to be convinced that it has no advantages. It may be well to state in conclusion, that the prevalent type of fever has been what would be called "typhoid," not true "typhus;" one or two had the aspect of congestive typhus, but wanted the purple-mottled rash."—*St. Louis Medical Journal.*

ON THE EMPLOYMENT OF THE CHLORATE OF POTASS IN MERCURIAL STOMATITIS, AND IN OTHER DISEASES OF THE MOUTH.

BY M. J. V. LABORDE.

We give the conclusions arrived at in this interesting and important memoir, which obtained the Corvisart prize for 1857.

The chlorate of potass exercises a *distinctly curative* action. In no case tested was this action found deficient.

It possesses, besides, a preservative, or prophylactic action, which permits of the administration, during one or two months, of the protiodide of mercury in doses of from fifteen to twenty centigrammes a day without the least injury to the cavity of the mouth; and the proof of such immunity being due to the chlorate of potass, is found in the fact of mercurial stomatitis being at once developed upon leaving off its use.

The time required for treatment of confirmed stomatitis varies with the intensity of the case. In ordinary instances, it never exceeds four days; in some extraorninarily severe cases, eleven days have been required. In almost every case, the first indications of amendment have shown themselves on the second or third day; and, according to what has been observed, the order in which these manifest themselves is—1st, in diminution and

entire relief from pain; 2d, in diminution of the salivation, and of sub-maxillary or parotidean swelling, when this is present: and, 3d, in the almost simultaneous disappearance of swelling of the gums, and a return to their natural color with the disappearance of ulceration where that has not existed previously to the stomatitis, in which case, of course, the chlorate of potass is ineffectual.

The increase of the dose does not seem to add to the rapidity of cure, except in very bad cases. In those of medium severity, the dose of three or four grammes is sufficient; and its administration in the form of a julep is preferable to all other modes.

When employed as a gargle, it seems very effectual in cases where the symptoms are exclusively local, such as in mere swelling of the tissue of the gums, their discoloration, ulceration, etc.

Its action appears to be entirely localized to the mouth, as is evinced by its being found in the saliva, as well as its effects being exhibited in the mouth when administered internally; and it appears to act as a sort of specific against the effect of mercurial inflammation in this quarter. At the same time it does not interfere with the therapeutic action of this remedy.

Of nine cases of chronic gingivitis, eight of which were accompanied with alveolar suppuration, six cures were effected under its use; two cases were unsuccessfully treated in the same manner; and the one case, without suppuration, was cured only after a prolonged course of treatment.

In only one of these successfully treated cases of gingivitis was the remedy given internally. The cure was complete in twenty days, and the first symptoms of amelioration were manifested about the seventh day. In five other cases, in which the remedy was given internally, no effect was produced by it; but on being used as a gargle, its good effects were speedily apparent — four or five grammes at a time, used in this way, leading to recoveries in from three to eleven days.

The good effects of such treatment almost always begin to manifest themselves in these cases about the second day, and consist at first in a well-marked improvement of color in the gums, the diminution or cessation of purulent discharges, and

the relief of pain. Even in the most obstinate cases, the swelling, discoloration, and pain soon subside—the suppuration alone being difficult to deal with. And in every case the dental tissues themselves rapidly recover their normal whiteness, however much this may have been altered.

In conclusion, it may be remarked, that the chlorate of soda very closely approximates in its qualities the chlorate of potass; its solubility is greater, and its taste less disagreeable. Both are therefore applicable and efficient remedies in acute ulcerous stomatitis.—*Va. Med. and Sur. Journal.*

ACTION OF MERCURY ON THE SECRETION OF BILE.

(From the Boston Medical and Surgical Journal.)

Among the effects of medicine which are usually considered to be well established, the action of mercurial preparations in increasing the flow of bile has been admitted without question. Physicians speak about stimulating a sluggish liver by blue pill, or calomel, as if it were not only the easiest thing in the world, but as if we had ocular demonstration of the process; and the frequency with which the liver is thus stimulated to an increased secretion of bile is in proportion to the ease with which it is supposed this may be effected. No matter what the disease, it is the commonest thing to begin the treatment by an active mercurial purgative.

Some experiments by Dr. George Scott, of Southampton, England, made on dogs, with a view of ascertaining whether mercury really increases the flow of bile, lead to the conclusion that the hitherto-received opinions on this subject are erroneous, and that calomel at least does not increase the biliary secretion. Having ascertained the average quantity of bile secreted in twenty-four hours, by collecting it in a vessel, after the common duct was tied, Dr. Scott administered calomel to the dogs, and then noted the amount of bile, the quantity of food and drink taken being the same. The four experiments of Dr. Scott all gave the same result,—that *there was a diminution in the amount of bile secreted after the administration of large doses of calomel.*

If these experiments should be confirmed by future ones, a revolution may be expected in the treatment of diseases supposed to be connected with a deficiency in the biliary secretion; and that much-abused organ, the liver, will be allowed some rest from the incessant appeals which are made to it as the source of so many functional diseases. We are glad that Dr. Scott has undertaken to investigate the effects of calomel on the liver by direct experiment. We hope he will continue his researches, and extend them to other subjects connected with the action of medicines. There is no department of our science of which so little is known, or in which more light seems capable of being thrown by direct experiment.

Surgery.

ON THE RADICAL CURE OF REDUCIBLE INGUINAL HERNIA.

(From the Nashville Record.)

The radical cure of inguinal and scrotal hernia, long regarded as almost hopeless, is now reduced to a fixed fact, and is numbered among the most successful labors of the surgeon. The profession is indebted to the labors and ingenuity of Professor Wurtzer, of Bonn, for the suggestion and perfection of this operation. 'Tis strange that he could have practised it for twenty years, and the profession of England and France should have remained in ignorance of it until 1854: it was in the earlier part of the same year that it was first introduced into the United States.

The first operation performed in the South-west was by Professor May, in Nashville, last November, before the class of Shelby Medical College. Since that time it has been frequently performed by Prof. May, Dr. Newman, and myself, with satisfactory success.

I first used Wurtzer's instrument: the objection to it is the inability in all cases to adapt the cylinder to the size of the abdominal openings and the inguinal canal. The Rothman instru-

ment is a modification of Wurtzer's, but possesses the advantage of sliding-pieces, by means of which it can be adapted to all cases, making it decidedly preferable. The objection of weight urged against this instrument in one of the following papers is obviated by having it made of hard rubber, such as is now constructed by Tiemann & Co., of New York. The manner of performing the operation is simply to invaginate before the finger of the left hand a portion of the scrotum, through the external abdominal ring, and along the inguinal canal as high as the internal ring; then, having well oiled the cylinder of the instrument, take it in the right hand, and gradually introduce it behind the finger while it is being withdrawn. Be sure the instrument has entered the canal, and is well pushed up to the internal ring; then the needle is passed through the cylinder and out through the aponeurosis of the external oblique muscle and common integument. The clamp is now applied with pretty firm compression. The point of the needle and its handle are removed, and small metallic balls substituted in their place. The scrotum is prevented from dragging upon the instrument by a scrotal supporter. The operation is now completed, and the instrument permitted to remain from seven to ten days. The patient is required to remain all this while in the recumbent position, with the head a little elevated, and the thighs flexed upon the abdomen, which is the position for the operation; and even for a week after the instrument has been removed, a spica bandage and compress being kept applied. When the patient gets up he should wear a light truss, well applied, and do only light work for several months, when the cure may be relied upon as radical. I cannot insist too positively upon the necessity of wearing a truss for several months. The only case of failure which has come under my observation has resulted from negligence in this particular. It is easily understood how the adhesions of the plug, while yet tender, may be broken up unless they have some artificial support, but after having had time to become firm, it is easier to force a rupture upon the other side than to have a recurrence of this. T. L. M.

NEW MODE OF RELIEVING RETENTION OF URINE.

(From the British Medical Review.)

Dr. Langstone Parker states that he has very recently succeeded, in two separate instances, in relieving retention of urine in the following manner:

"A gentleman lately entered my consultation-room in great pain from retention of urine. He had not passed water for many hours; the bladder was much distended. He stated that ineffectual efforts had been made to pass a catheter, during which operations he had lost a considerable quantity of blood. I attempted to relieve him by the catheter, but failed to do so; I tried instruments of various sizes and various curves, but could not succeed in passing one into the bladder. I then took a number two wax bougie, and inserted a small portion of potassa fusa into the end of it, after the manner proposed by Mr. Whately, and practised by Mr. Wade, in the treatment of permanent stricture of the urethra. I well moulded the wax over all but the extreme point of the caustic, and passed it rapidly down to the point of obstruction; by pressing against this for a short time, it yielded, and I had the satisfaction of finding the bougie easily enter the bladder. I directed the patient to strain as I withdrew the instrument; a stream of urine followed, and the bladder was emptied. The retention did not again occur, and very little irritation accompanied or followed the proceeding. On the next day the patient made water freely, but in a small stream.

"The second case was very similar. The patient had travelled some distance by rail. The bladder was much distended, the symptoms urgent, and a catheter could not be made to enter the bladder. A small wax bougie was armed, as in the last case, passed down to the stricture, and firmly pressed against it. It yielded very quickly; the instrument entered the bladder, and a stream of urine followed its withdrawal. The patient had a second attack of retention two days afterwards, which was completely relieved in the same manner.

"A modification of this plan might be attempted by inserting a small piece of potassa fusa into the extreme point of a small gum-elastic catheter, and using it without the stilette. I am

sanguine enough to hope that many cases of retention of urine might be easily and quickly relieved by the simple means I have suggested, and more formidable and dangerous operations thus frequently avoided."

A NEW DISINFECTANT.

At the meeting of the Academy of Sciences of Paris, of the 18th July (Gazette Hebdom., July 29th), MM. Demeaux and Corne, two former internes of the Hospice de la Charité, made a report on a new disinfectant, which, if all that is claimed for it is true, is destined to effect a great amelioration in the treatment of ulcers, abscesses, flesh wounds, etc. The object of the discovery is the complete and instantaneous disinfection of animal matter. They claim that it arrests decomposition, and effectually prevents the generation of insects. The following is the formula given by the inventors:

Plaster of commerce, reduced to a fine powder, 100 parts. Coal tar, one to three parts.

The mixture of the two substances is effected with ease by the aid of a mortar, or any other appropriate mechanical means. The mode of application is the following: A certain quantity of the powder is diluted with olive oil to the consistency of paste, or ointment, which is of a dark brown color, has a slightly bituminous odor, and may be kept in a closed jar for an indefinite period. The oil unites the powder without dissolving it, and the composition has the property of absorbing infectious liquids the instant it is applied to the sore which produces them. The application may be mediate or immediate. In the latter case no pain whatever is produced; on the contrary, the salve has a detersive action, cleanses the sore, and favors cicatrization. MM. Corne and Demeaux state that the composition may be applied in the form of a poultice, or on cotton, and laid on the wound. They demonstrate that their mode of dressing possesses the double property of disinfecting morbid products, and of absorbing their liquids. The last circumstance entirely obviates the necessity of lint, which is an important feature of the discovery. The

communication was referred to MM. Chevreul, Velpeau, and J. Cloquet.

At the meeting of the 25th July, M. Valpeau made a report of his experience with the new disinfectant, during the short time that had elapsed. He was quite favorably impressed with it, having applied it to several cases in which the discharges from ulcers or wounds were highly offensive, with the effect of immediately correcting the odor, and improving the appearance of the wounds. The testimony of others was also given in favor of the new disinfectant.

At the succeeding meeting of the Academy (August 8th), the subject was again brought up, and a report read from Baron Larrey, surgeon-in-chief to the army of Italy, who had a trial made of the new disinfectant, and who reports favorably in regard to it. It would seem to be worthy of the attention of the profession of this country.— *Virginia Med. and Surg. Journal.*

NEW METHOD OF TREATING POWDER-WOUNDS.

BY PROFESSOR BUSCH, OF BONN.

Instead of treating powder-wounds by the painful process of digging out each single grain of powder with the knife or needle, Professor Busch recommends the fomentation of the wounded part with a strong solution of corrosive sublimate (gr. v. to ℥ j): this application produces an eczematous inflammation; some of the vesicles simply dry up, others form scabs. On removing such a scab, the grains of powder are seen to adhere to its under surface, and underneath it a newly-formed, spotless epidermis is found. The scabs and epidermis scales, together with the grains of powder, may then be scraped off with a spatula.

Any other strongly irritating application may be used with success; the solution of sublimate recommends itself the most, because in using it the degree of irritation can be controlled pretty accurately, and because, after the healing of the eczema produced by it, a white skin remains.— *Virch. Arch. f. Pathol. Anat.*

Editorials and Miscellaneous.

THE MEDICAL BILL.

The Virginia Medical Journal is opposed to the bill creating a Board of Medical Examiners for North Carolina, upon the ground that it encourages quackery, and thus defeats its own objects. As to the precise method in which this is to be accomplished, we are left to conjecture, for, with the exception of much declamation respecting "the diploma qualification," "unwise legislation," &c., the article in question seems to be nothing more than a glorification over a Bill proposed by the Medical Society of Virginia.

As we desire to do our contemporary the fullest justice in this matter, and are anxious to meet his objections to the bill fairly and fully, we will examine the positions assumed by him, in detail.

(1). As to the diploma qualification. It is urged, as an objection to the bill, that "there is no requirement of the applicant that he hold a degree." This argument, though exceedingly specious, is nevertheless sophistical and unsound, as can be easily demonstrated. A diploma *can be* nothing more than an evidence of qualification on the part of its possessor, and *should be* nothing less. This *qualification* does not follow as a necessary sequence upon the "attendance of lectures," the "dissection of dead bodies," the "witnessing of surgical operations," &c.; but can only be ascertained through the instrumentality of an examination. A diploma, then, which has been legitimately obtained, indicates that its recipient has been subjected to such a thorough test, as gives positive assurance of his qualification to practice Medicine, and thus becomes *ipso facto* the accredited witness—the representative of that examination to the world. From this plain statement of facts, it becomes evident, that however valuable this type, symbol, or representative may be, *per se*, it sinks into a position of *subordination* when contrasted with that examination of which it is an exponent, and through the immediate instrumentality of which the question of qualification is effectually settled.

Our Medical Bill demands a rigid examination of the claims of each candidate before granting him permission to practice within the limits of North Carolina, and thus substitutes a positive and superior test for the inferior and subordinate one embodied in a diploma. It is manifest, then, that the only injury which can result to the Profession in this connection, is from a want of fidelity among the members of the Board in carrying out the provisions of the bill,—a result which, though most deplorable in itself, would reflect in no degree upon the Bill, since fallibility is an inseparable attribute of human agency. The principles enunciated above cannot be affected in any degree by the weakness and corruption of those entrusted with their application. The Board may be prostituted to the basest purposes without compromising the wisdom of the law creating it. The Decalogue will command the respect and admiration of the world though its every precept be broken to the end of time.

But there is another view of this subject equally important to the question at issue. We have hitherto spoken of what a diploma should be, and not what it is. It is unnecessary to attempt a formal argument for the purpose of demonstrating how and why the standard of medical education has depreciated in this country, for the fact is as universally acknowledged as it is deplored. To so great an extent has the rivalry between opposing schools reduced the standard of qualification among medical graduates, that the possession of a diploma is no longer regarded, either by the profession or the people, as a positive evidence of merit or respectability, but as simply conferring the title of Doctor; and in this estimate of the value of such a document, they are not only sustained by the action of the Army and Navy Boards in rejecting annually a large number of medical graduates, and by an acknowledgment on the part of the National Medical Association of the absolute necessity of some reform in the method of granting diplomas, but by the course of the Virginia State Medical Society—of which our friend is a member—in proposing a bill which requires the *re-examination* of *all who desire to practice Medicine in that State.* If, then, we have such high authority for regarding a diploma as SOMETHING LESS

than an infallible evidence of qualification, it must follow that the mere possession of such an instrument should confer no special privilege or prerogative, and that it is fair and right to regard all who are actually qualified — whether titled or not — as equals in every respect. The framers of our Bill, prompted by an ardent and most commendable desire to secure competent men for the ranks of the profession in North Carolina, and feeling thus assured that others than the holders of diplomas might be fully equal in medical knowledge to their titled brethren, determined to grant to *all* the privilege of standing a thorough, *searching* and *satisfactory examination*, and to give to those who were found qualified a certificate, or license, as an evidence of that fact.

We readily admit, that though a diploma is not a certain criterion of proper medical education, it is an evidence that a course of study has been gone through with, and that its recipient has had an opportunity of preparing himself. This being true, and feeling the necessity for drawing some line of demarkation between the regular and irregular followers of Æsculapius, we have always insisted on the "diploma qualification" as a *sine qua non* of membership both in our State and the National Medical Association; and it was with deep regret that we witnessed the contempt manifested for diplomas by the latter body, at its recent meeting in Louisville.

There is, however, a vast difference between demanding this qualification as a pre-requisite for membership of a scientific body, and requiring it as an essential condition to the granting of a license by a medical board. In the first instance, the diploma is the only standard of attainment by which a man's claim can be tested, and though exceedingly unreliable, it is better than none, since it shows that the individual *may be qualified.* In the second, we have an absolute test of preparation, &c., in the rigid examination given, which obviates the necessity for the inferior standard, makes merit the platform upon which the claims of every worthy aspirant are equalized, and demonstrates that the individual *is qualified* for the responsible position he desires.

Again, what is the difference between a diploma and a license,

that such an arbitrary discrimination should be drawn between them? A diploma is conferred by the authority of the State — so is a license. A diploma is presumed to be an evidence of qualification — so is a license. A diploma gives to its possessor the dignity and title of a Doctor — so does a license. A diploma bears upon its face the broad seal of a corporate body, and, as such, gives a legal position to the graduate — so does the license. And more than this, the representatives of the colleges themselves are uniting with the laity in depreciating the dignity of the diploma, by compelling their graduates to submit to a new examination, and to be *licensed*, before practising their profession — even in Virginia. It is true that, in the one instance, the power of granting the degree resides with those who teach; whilst in the other, the licensing and teaching power are separated. But the interests of the profession,— the advancement of science,— the claims of humanity, demand proper qualification on the part of medical men, and ask no farther questions. So that the physician understands his duty,— so that he is prepared to exercise his vocation,— so that the sacred cause of Truth advances,— so that lives are saved and suffering alleviated, who cares how, or when, or where the knowledge was obtained, or by whom the right to prescribe has been conferred? If a man be thoroughly qualified, is his diploma less valuable because his knowledge has been acquired in some other locality than that wherein his honors and titles were received? Where is the necessity for the least connection between the process of teaching, and that examination through which the student shows his proficiency, and wherein he acquires his right to a license or a diploma, or whatever it may be called? Fortunately, there is a means of obtaining a practical answer to these questions, and we will only refer our readers, in this connection, to the honor and success which have crowned the labors of the independent Boards of the Army and Navy in the cause of Medical Education. These Boards assemble annually, and, without asking a single question as to the length or extent of the curriculum pursued, and demanding only the most accurate and comprehensive acquaintance with Medicine in all of its varied departments, they

grant licenses, or commissions, to such as pass a satisfactory examination. It is hardly necessary to inquire if quackery is encouraged, or the profession injured, by such a course as this — a course wherein the "diploma qualification" is utterly repudiated, and which subordinates everything to the more satisfactory test of a searching examination? It is universally acknowledged, that in point of scientific attainment, and in all that elevates and adorns professional character, the medical departments of the Army and Navy have no superiors in this or any other country. We desire to raise the standard of attainment in Carolina, just as it has been done in the Army and Navy, and our bill has been framed with especial reference to that object — an object most commendable in itself, and which experience demonstrates to be attainable by the very means our worthy friend regards as destructive of the best interests of the profession.

Our honored cotemporary involves himself in an inconsistency, it seems to us, when he attempts a fling at the substitution, on our part, of "an Examining Board, in the place of the examinations of Mott, Parker, Bache, Jackson and Dickson." Now, if there be anything wrong, or detrimental to the profession, in this substitution, it must consist in the attack which is thus made — not upon their ability to examine properly, for that would be absurd,— but upon the fidelity with which they perform their most responsible duty. This is the only thing implied in the course pursued, and if any injury be done at all, it must be in that particular. If Professors examine properly, and grant diplomas only to those who merit them, then must the ranks of the profession be filled up with *worthy men*, and no just cause of complaint can possibly exist against the present system of medical education. But, as it is a notorious fact that the system is defective, and that *unworthy men* are constantly invested with the prerogatives of the Doctorate, it must follow that the schools —that "Mott, Parker, Bache, Jackson, Dickson," *et id omne genus* — do not examine their candidates properly. This being true, it is evident that the attack upon them, implied in *the substitution* above referred to, so far from being injurious to the profession, must really prove of decided advantage to it, by ex-

cluding the ignorant and unworthy, and admitting those only who are capable of discharging its most honorable duties. It is plain also, that the proposition to appoint medical boards, from whatever source it originates, or wherever made, is based upon the fact that the schools do not examine their candidates strictly, and is an acknowledgment that "Messrs. Mott, Parker & Co." are derelict in their duty. This is the plain English of the whole matter; for, if the original test be an adequate one, where is the least necessity for a re-examination? And yet, strange as it may appear, the Editor of the Richmond Journal, though amazed at the temerity of an attempt in Carolina to "substitute a medical board for the examination of Mott, Parker & Co.," actually advocates the passage of a bill in Virginia, which compels all who have been examined even by these distinguished men, to be *re-examined* by a board appointed for that purpose, before "*presuming* to practice Medicine" in the Old Dominion. It is utterly impossible to comprehend so paradoxical a position as this; and with this exhibition of our friend's inconsistency, we will leave the whole subject of "diploma qualification" for the present, believing we have disposed of the objections to our bill, urged in this connection.

(2). As to the appointing power. Our friend affirms, that, under the provisions of our Bill, the members of the Examining Board are to be appointed by the State Legislature, and urges this as another objection to the Carolina law. To this, we will simply answer, that we have at this moment a board of examiners, whose authority is not questioned, and which was duly elected *by the State Medical Society at its recent meeting in Statesville.*

Had our cotemporary read the Bill a little more attentively, he would have discovered, that though the Legislature reserved the *right* of selecting the members of the Board, it empowered the State Society to act for it in this regard, whenever it failed to make the appointments. As there was a tacit understanding among the members of the Legislature that the selection should be made by the Profession, and as it adjourned without going into an election, the only thing remaining for the Society was, to

appoint the Board, which it accordingly did, at the time and place designated. Thus does another of the arguments so specially employed against our Bill fall to the ground, utterly paralyzed by a bare statement of the facts of the case.

That the Legislature did not reserve the appointing power for any improper purpose, is established by the fact, that it permitted the Society to elect members of the Board, thus securing, for six years at least, all that the Profession could ask at its hands.

The truth is—and we speak *ex cathedra*—the bill was drawn up by a committee of medical men appointed for that purpose, who labored most industriously to meet every contingency wherein its success might be jeopardized. Warned by the sad spectacle which presented itself in Virginia and other States, these gentlemen supposed it possible that even so stable an organization as our State Society might dissolve in the lapse of time; and, desirous of perpetuating an institution for which they had labored so actively, they reserved for the legislature the power of appointing the Board, so that it might be exercised in such an emergency as we have indicated, and the advantages accruing from this most important measure, permanently secured to the profession and the people of North Carolina.

We know well that legislative appointments are generally influenced by political prejudices, and for that reason, particularly, are objectionable: but as such things have not always been, and a better day may yet dawn upon the Republic, the motive was commendable, and the stake worth the effort thus made to secure it. And yet, admitting that the political bias influencing legislative bodies militates most seriously against the interests of Medicine, and acknowledging the force of the objections urged against this method of appointment, we can but express the most unfeigned astonishment that our friend of the Virginia Journal should make such an attack as this on the Bill. We say this because, in reading the proceedings of the Medical Society of Virginia, we find that, after Dr. Tucker had inveighed against the policy of introducing a political element into the Board, by conferring upon the Executive the power to make some of the

appointments, our worthy friend actually proposed, as an amendment to a resolution offered as an expression of Dr. Tucker's views,—that the governor be empowered to appoint three members of the Board,—though assured that partisans would receive the honor.

To sum up: the Examining Board is in existence,—having been appointed *by the Medical Society of North Carolina*, and not by the Legislature; whilst, if such were not the fact, and political bias could come in to influence the election of its members, the Editor of the Virginia Journal has no right to utter a word of condemnation, as he has introduced into the Bill proposed by his Society a feature which is essentially and avowedly *political.**

(3). As to the penalty. It is farther affirmed that the profession is injured by the bill, because a violation of the law cannot be esteemed a misdemeanor.

Now, it cannot be denied that a bill would be more efficient and complete if, as is proposed in Virginia, it imposed a heavy fine on those, who practised without the sanction of the Board; but it does not follow that the law is a nullity because such a provision has been excluded from it. We had been struggling for a long period to secure the passage of some bill for the protection of the profession, and the people against quackery and imposition, and had found—as our Virginia brethren will find—

* Extract from the Minutes of the Virginia Medical Society:—

"The sixth section was then taken up, which declares that the governor shall appoint the members of the Board.

"Dr. Tucker strenuously opposed this feature of the bill. He deprecated political influences in political matters; had no respect for political doctors.

"Dr. Tucker moved that the Board be constituted of one member from each of the three medical colleges of the State, and six other medical men, not professors, but chosen by the three professors above mentioned.

"Dr. Parker moved that the Board be constituted of one from each of the three medical colleges, and the remaining six be appointed by the Medical Society of Virginia.

"Dr. Bolton supported this motion.

"*Dr. McCaw* moved as an amendment to Dr. Parker's resolution, the following: 'That the Board be composed of three members appointed by the colleges as aforesaid, three by the State Society, and the remaining *three by the governor*, not professors in medical colleges.' "

a legislative body exceedingly difficult to manage, at least so far as the interests of the profession are concerned.

After years of patient labor, when nearly overwhelmed by the difficulties which presented themselves, both from a want of unity among the members of the profession, and from the indifference or cowardice of the legislature, the committee appointed by the State Society to frame a proper law on the subject, found it in their power to secure the passage of "*the* Bill" which our friend considers so emasculated and impotent, and of *none other*. They had to choose between surrendering, definitely and disgracefully, after a thirty years' fight, and securing a victory which, though it may be only partial, is a triumph after all. They had to decide between leaving quackery alone in its glory, or raising a barrier against it, which, if not of sufficient altitude and strength to dam up the current completely, still retards the stream, and furnishes a firm basis for future operations. In a word, they were called upon to say whether there should be no Bill, or one which, though it does not impose a fine, prevents the collection of debts due to the unlicensed, gives only to such as have been properly examined the sanction and recognition of the law, and places every presumptuous pretender, whether titled or not, beyond the pale of professional sympathy and intercourse. These were the questions which the framers of the bill had to decide, and we boldly ask of every candid man, whatever his locality, if, in pursuing the very course which has called forth the censure of our cotemporary, they did not act just as good sense and the highest interests of the profession imperatively demanded at their hands?

That the Bill would be *better* were a pecuniary penalty imposed, we will not question; but that it is *bad* because of the absence of such a provision, we not only deny emphatically, but will prove by the admissions of the Virginia Journal itself. Whatever the guise which quackery wears, or wherever it may grow, it originates in nothing but a desire for money. In proportion, then, as the gratification of this desire is rendered certain or otherwise, quackery will flourish or languish in any given locality. If our bill makes the acquisition of money by the unli-

censed *more certain* in Carolina, then, as a matter of course, it "encourages quackery"; whilst on the other hand, if it render the process of money-making *less* certain, it follows necessarily that it restrains quackery to that extent.

Our Bill, as previously stated, relieves the person who has received medical attention from an unlicensed physician, of the legal obligation to pay for the service rendered. We will not stop to argue the moral points involved in our friend's question, "What is this penalty worth?" but will only say that it is worth just what *legal protection* invariably is, and nothing less. Human experience demonstrates that, without law,—without legal sanction and protection,—without that security which the strong hand of some paramount, potential, and acknowledged authority affords, all justice and obligation,—all the bonds which bind men in communities, become valueless and unreliable, whilst, instead of that condition of peace and security, which is the final cause of every governmental system, there results that state of anarchy wherein the natural depravity and inherent selfishness of mankind manifest themselves in their greatest intensity, and which is the very embodiment and illustration of *uncertainty* itself as regards all the relations of life. It may be then assumed as an axiom, that, where there is no legal protection, and the caprice or selfishness of human nature asserts its unholy sway, there must result confusion, contradiction and uncertainty, as a matter of necessity. Our legislators have removed this legal protection from those who are not licensed by the Medical Board of North Carolina, and, according to every logical principle, historic precedent, and natural law, the collection of the debts due to such persons must be rendered more *uncertain*, than to those over whom the law throws the ægis of its protection. As, without the collection of their debts, the acquisition of money becomes an impossibility, it must follow, from the *uncertainty* thus demonstrated, that *quackery* is discouraged by the Carolina Bill.

This view of the subject is still farther strengthened by the fact, that even with all the protection the law affords, the experience of the world proclaims that medical accounts are, of all others, most difficult to collect, and that, in despite of the power

of statutes and the appeals of gratitude, the physician, as our friend admits, "generally finds it to his interest to settle with his employer on the best possible terms." If the collection of accounts be thus *uncertain* even under the most favorable circumstances — when the legal obligation, and the promptings of gratitude, both combine to support the claims of the physician, does it not follow, if so potential an influence as that which the law supplies be permanently removed, and nothing remains as a motive power save that uncertain, unreliable, and most capricious sense of gratitude pervading the human heart,—that the *uncertainty* of collection increases, and that quackery is necessarily restrained thereby?

Even though it "rarely happens that a suit is instituted by a physician to collect his bill," nothing is proved thereby in opposition to our position. The fact that such sights are *some* times witnessed, whilst demonstrating the ingratitude of those who receive medical attention, and thus making the "uncertainty of collection" still more apparent, establishes the necessity for "legal protection" in extreme cases at least, in order to prevent the perpetration of injustice and wrong. And yet we are told, that the abolition of this "legal protection" is no hardship at all, although any member of the profession is liable to be thus "injured and wronged" in the vindication of his indisputable rights.

Again, does it follow, because "not one in fifty ever appeals to the law," that without "legal protection" the collection of debts would be as certain as it is now? In order to obtain a proper answer to this question, it is necessary to propound another. If one patient in a thousand, or ten thousand, so far forgets his legal and moral obligations as to refuse to pay for medical attention, and thus constrain his physician to institute a suit against him even with a knowledge that the law of the land stands ready to force him to a final settlement, would not the number of such delinquents be immeasurably increased by an entire abolition of the law? If the inherent injustice of human nature be so difficult to control, that even the strong hand of the law is incapable of the task, would not its manifestations increase in number and potency with the total withdrawal of all restraint?

Common sense gives but one response to these interrogatories: and with the affirmative answer thus supplied, there is evolved the *fact* that "though not one in fifty appeals to the law" under existing circumstances, the necessity for such an appeal would increase beyond all calculation in the event of the abolition of the law. Because not one person in fifty commits robbery or murder under the law, would it be esteemed no "hardship" to society to abrogate the law, and to trust to human nature to regulate itself? It is manifest, then, from this view of the subject, that the withdrawal of "legal protection" from the unlicensed, is a most serious matter, and well calculated to deter the charlatan from pursuing Medicine as a vocation.

There is still another view of the matter, worthy of consideration in this connection. Physicians do not appeal to the law for two reasons: the first is, that they prefer, as a matter of policy, to trust to the combined influence of the justice of their patrons, and the knowledge of their legal obligation in this regard; the second is, that as soon as the first legal steps are taken, or even when they are seriously threatened, the necessity for farther measures is usually obviated by the payment of the debt unless there be some dispute about the amount claimed, or the debtor desires delay for some special reason. If the legal obligation were removed, and nothing but the justice or gratitude of patrons left to depend upon, it is manifest that they would be compelled to trust to "a broken reed and a rope of sand." The knowledge that there is a superior authority from which there is no appeal is, then, the motive power which thus, either indirectly or directly, influences to the payment of a physician's dues, and prevents him from actually bringing suit. It is because he *can* appeal to the law with the certainty of redress,—because the means of forcing a settlement *are placed* at his command,—because the State stands ready to sustain him to the whole extent of its power in the collection of his debts, that the legitimate son of Æsculapius feels protected in the discharge of his duties, and is preserved from that injustice and wrong which mankind delights to mete out even to its greatest benefactors. It is this that induces a consciousness of security in the pursuit of our

most noble vocation, whilst it secures a proper remuneration for the services so liberally rendered by the Profession. And it is this which obviates the necessity for appealing directly to the courts, and renders a suit by a physician so unusual an occurrence.

And yet, in view of these facts, our friend affirms that the withdrawal of this protection, with the insecurity and uncertainty resulting therefrom, so far from diverting the unlicensed from the pursuit of Medicine as a means of support, actually encourages quackery, and militates against the best interests of the profession.

However seriously this may affect the physician himself, it operates far more injuriously upon others connected with him. He may manage to cajole his patrons into making some return for his labors in their behalf, even without the stronger argument which the law supplies; but the widow and the orphans he leaves behind him, with nothing but the cold sympathy and heartless gratitude of the world to depend upon, have a lot that is terrible enough in itself, but which is rendered still harder by the reflection, that it has been deliberately entailed upon them by him who should have been their protector and their friend; for, though thousands might be justly due for services faithfully rendered, the family of the unprotected physician would have no legal right to a single copper; and the history of the world has to be written anew, if the weak and helpless find other security than that afforded them by the laws of the land. We will simply ask our cotemporary, if he would esteem it no hardship thus to *provide* for those who are nearest and dearest to him? Is not the reflection that a beloved wife, and a cherished family, may be thus thrown upon the cold charity of an unfeeling world, enough to deter any man in his senses from the pursuit of a vocation over which the law throws no shield whatever? And yet, all who presume to practice Medicine in Carolina, without a proper license, expose themselves to just such a contingency as this, and with only the probability of an inadequate support to lure them into so treasonable a forgetfulness of the tenderest ties of nature.

The man who practices without "legal protection" must be particularly careful, too, only to select favorable cases for the exercise of skill, since if, by an accident, he lose his patient, he makes the collection of his bill, in many instances, a matter of impossibility; for it is well known that no executor, administrator or guardian can settle any bill, save one recognized by the law, except at his own risk; and hence proper legal steps have to be taken, as a matter of necessity, by every physician who attempts to collect a debt under such circumstances. If the accounts of an unlicensed physician are not recognized by the law, they must be abandoned, since the proper measures for their recovery are utterly precluded in this connection, to the serious detriment of their possessor.

As practitioners generally find some difficulty in getting hold of fortunate cases only, and as all who pretend to prescribe for the sick must rely, very frequently, upon these legal representatives for the *quid pro quo*, it follows that the loss of such claims is a most serious matter—such a hardship, in fact, as would be felt and fully appreciated by more "than one in fifty" of the profession. But this is one of the penalties imposed by our *emasculated* (?) Bill, and if it be not a serious one—if it be not a most potent restraint upon the *unlicensed*, they must practice for something else than a pecuniary consideration.

There are other disabilities connected with this "insufficient" penalty, which our cotemporary has not considered, and which render it more stringent than he supposes.

The regularly licensed only are to be regarded, either by the State, the people or the profession, as physicians. The unlicensed are to have no legal recognition or existence. They are to be deprived of all the exemptions and privileges which the State accords to professional men. They are to be forced into such a position of inferiority and subordination to their more honored brethren as is sanctioned and defined by the civil authority—as the law of the land pronounces *prima facie* evidence of ignorance and incapacity, and from which there is no escape save by a strict compliance with the provisions of our Medical Bill.

The people of North Carolina have solemnly declared, through

their representatives, that all who refuse to submit to such an examination as they deem requisite, are unworthy of their confidence, and deserve public reprobation. They have marked them with the sign of dishonor. They have formally ostracised them as quacks, and repudiated them as pretenders. They have placed the seal of their condemnation upon them, and openly declared them impostors and charlatans; and from the judgment thus pronounced there can be no appeal until prejudice has triumphed over the best interests of Medicine, and the Medical Bill has been repealed by the Legislature of the State.

The regular members of the profession esteeming them inferiors both in knowledge and standing,—ignoring their claims to the rights and privileges of the Doctorate, and regarding them as quacks and pretenders, will feel under a most imperative obligation to refuse them all recognition as brethren, and to hold them up to the contempt and condemnation of the public. Whoever refuses to submit to the examination prescribed by our Medical Bill thus arrays against himself the combined influence, the united opposition, the entire moral weight of the whole profession,—and with nothing to relieve the odium of his position save the possibility of realizing an insufficient income.

Now, we beg leave to ask, if a penalty which involves the delinquent in so humiliating, embarrassing, and painful a predicament,—which consigns him to such a position of degradation and inferiority,—which thus seals, marks, and ostracises him as unworthy of confidence and respect,—which arrays against him the unmitigated reprobation of the whole medical public, is not a stronger barrier against the *unlicensed*, and more of a safeguard to the profession, than the Editor of the Virginia Journal seems to regard it?

From all these considerations, it becomes evident, that though the penalty imposed by this Bill may not be as potential as desired by the friends of the movement, it is far more serious in itself, and efficient in its operations, than our cotemporary supposes, and that in this particular, as in many others, he has made a mistake which does the Profession of this State the greatest injustice.

EXCHANGES.

We feel under many obligations to the Editors of those Journals which have so kindly exchanged with us, and sincerely hope that they will continue their favors in the future.

Nothing renders us more sensible of our own deficiencies than the admirable manner in which our Exchanges, generally, are conducted. They are, in fact, an honor to the profession and to the country, and their editors deserve the highest praise for the energy and fidelity displayed in their pages.

For the many flattering things they have been pleased to say of the "Medical Journal of North Carolina," we return our grateful acknowledgments, assuring them that many an hour of anxious toil is compensated for thereby, and promising to prove more worthy their encomiums, if untiring devotion to our work can accomplish such a result.

But few of the newspapers of the country have complied with our request for "an exchange"; but whilst we are patronized by such valuable journals as the "Medical and Literary Weekly," the "Raleigh Standard," "Wilmington Herald," the "Leisure Hour," the "South Carolinian," the "Family Visitor," the Elizabeth City "State," and the Edenton "Express," we can afford to be overlooked by many others.

Among our most regular "exchanges," is the "North Carolina Journal of Education,"—a work which is an honor to the State, and well worthy the liberal patronage of her citizens.

The Editor of that most excellent paper, the "Family Visitor," published at Gatesville, N. C., will please accept our kindest acknowledgments for personal as well as editorial courtesies.

The Dental Cosmos.—This is the name of a new and most spirited Journal just started in Philadelphia, and devoted to the interests of the Dental profession. It presents a very neat appearance, and is evidently conducted with much ability. We gladly place it on our "list," and shall take pleasure in enriching our pages by frequent extracts from its columns.

A NEW VOLUME.

The Medical Journal of North Carolina has accomplished the first year of its mission, and now starts upon the second,—not weakened or dispirited as some of its cotemporaries, but stronger, more hopeful, and better sustained than ever. It is true that some delay has occurred in the publication of this number, but it has resulted neither from the lukewarmness of its friends, nor the slothfulness of its editor; and we are happy to state that arrangements have now been made which will obviate all difficulty in this regard for the future. Hereafter, the Journal will be issued regularly, from its office in Edenton, on the first day of every alternate month; and we sincerely hope to make up, by our future punctuality, for the irregularity which has attended its publication during the past year.

We have endeavored to make the "list of payments" complete, but as the professional engagements necessarily incident to what is well known as the "sickly season" have engrossed a large amount of our attention, to the exclusion of everything else, it is possible that the name of some one may be omitted: if this be so, we trust our friends will understand it, particularly as it is only necessary for a subscriber to *say* that the money has been duly *forwarded* to insure an immediate rectification of the mistake. In these matters, our friends must bear with us, for, with the best intentions and the most diligent labor, the proper management of a Journal involves so much that is arduous, exacting and absorbing, that it is almost a matter of impossibility to keep every department accurately posted up. A little experience has, however, taught us to systematize much better than formerly, and for the remainder of our editorial existence we promise to show ourself much more of a business man.

Whilst other Journals are complaining of the illiberality of their patrons, the "Medical Journal of North Carolina" can proudly point to a subscription list which is daily increasing,—to a receipt in full from every one to whom it has been indebted, and to a generosity on the part of its friends which can find no parallel beyond the limits of our own most noble State.

Independent of all personal considerations, we feel proud of the triumphant success of the Journal, because it demonstrates that though Carolina may be asleep, she is alive to all that is noble, generous and honorable, and that she is more glorious, —more worthy of respect and admiration, than some of her prouder sisters who sneer at her as the Rip Van Winkle of the Union, and esteem themselves superior because "wider awake" to their own interests and aggrandizement.

To those who have been disposed to attribute the delay in the issue of the Journal to negligence on our part, we can only say, that we have labored most diligently to insure its earlier appearance, and that the unfortunate irregularity has been a source of infinite distress to the Editor.

Medical College of Alabama.—The following gentlemen compose the Faculty of this Institution: Dr. J. C. Nott, Surgery; Dr. J. F. Heustis, Anatomy; Dr. W. H. Anderson, Physiology and Pathology; Dr. G. A. Ketchum, Practice of Medicine; Dr. F. A. Ross, Materia Medica; Dr. F. E. Gordon, Obstetrics and Diseases of Women and Children; Dr. W. J. Taylor, Chemistry, and Dr. G. Owen, Demonstrator of Anatomy.

ERRATA IN VOLUME I.

Page 414, line 17 from top, for "ignus," read *ignis.*
" 429, " 10 " for "generally asthenic," read *generally as sthenic.*
" 521, " 2 from bottom, for "Skoda," read *Skoda's.*
" 524, " 20 from top, for "professes," read *possesses.*
" 533, " 7 from bottom, for "pneumonia," read *pneumonic.*
" 537, " 8 from top, for "greatest," read *greater.*
" 540, " 14 " for "greatly," read *gently.*
" 543, " 9 from bottom, for "rhonchus," read *rhonchi.*
" 548, " 25 from top, for "though," read *those.*
" 552, " 8 " for "in extento," read *in extenso.*
" 556, " 11 " for "predicting," read *predicating.*
" 560, " 5 " for "sanguineous," read *sanguineous.*
" 566, " 7 " for "hypogastric," read *lumbar.*
" 568, " 13 " for "Horteloup," read *Herteloup.*

Therapeutical Record.

Elastic Tubes.—The use of small india-rubber tubes (*tubes de drainage* of the French) instead of lint tents, after opening abscesses, etc., has not yet become nearly so general as it well deserves to be. They serve an admirable purpose, and do not irritate in the least. They are much easier to get in than lint or cotton wool; and when once in, instead of closing up the aperture, enable the matter to drain away as fast as secreted, and thus allow the sides of the cavity to remain in contact, with a good chance of speedy coälescence. In the treatment of post-mammery abscesses they are invaluable. Tubing of the size which is the most generally useful, may be had in the shops at sixpence a yard. To remove the sulphur, it should be washed in warm water with a little soda. Dr. Savage, Mr. Price, Mr. Spencer Wells, Mr. Bowman, Mr. De Morgan and Mr. Henry, have all employed these tubes for some two years past, and speak well of their utility. Their use has already become very general in the Paris hospital.—*Med. Times and Gaz.*

Lunar Caustic Spots.—When the spots are of quite recent origin, liquor ammonia will dissolve them, but the most efficacious means to remove old spots, even in clothing repeatedly washed, we found in cyanide of potassium. If the black spot is repeatedly rubbed with the solid salt (or solution), and washed in water, every trace of nitrate of silver is dissolved. As cyanide of potassium is a very violent poison, much care is recommended.

Tetanus.—On the 22d she was seized with symptoms of tetanus, which soon assuming an important character, she was ordered chloroform inhalations six times per diem, without much amelioration, until the fifth November. The chloroformizations were then repeated only four times, and, after a while, three times and twice a day; the whole quantity of chloroform consumed amounting to eight ounces, of which some was lost during its administration. She completely recovered.—*Ann. Omodei.*

Opthalmia of Children.—Mr. Ballard (Lancet) has written an original paper upon the above subject. The generally received opinion that the disease is the result of contact with vaginal secretions is disputed—1st, Because the disease does not appear until several days after birth; 2d, Extreme cases of leucorrhœa, and yet no opthalmia, if the room was kept dark; 3d, No leucorrhœa, and yet opthalmia, the room being kept light.

The proofs in favor of light being the cause of the disease are—1st, Expose an infant to bright light, and you can prognosticate the occurrence of the disease; 2d, It never prevails if the child is kept in the dark; 3d, That many cases had been cured by obscuration only.

Ergot.—M. Deville says that it is almost always easy to determine the causes which have destroyed an infant *in utero*, such as premature delivery, vicious presentations, &c. When, he proceeds, none of these causes exist, and a mature, healthy child is born dead, presenting all the appearances of asphyxia, it may be declared that ergot has been given. Analyzing various documents collected during the years 1845 to 1848, in Paris, M. Deville arrives at this result: that out of 515 still-born children, 72, or about one-seventh, perished from ergot. Researches continued by M. Deville since 1848, show that the number of still-born children tends to increase every day, and that this increase is owing to the administration of ergot or to induced abortions.

Secondary Syphilis.—A commission, composed of MM. Velpeau, Ricord, Devergie, Depaul and Gibert, appointed to inquire concerning the contagious nature of the secondary accidents of syphilis, has decided (with the exception of M. Ricord, who reserves himself on certain points)—1. That there are secondary or constitutional accidents of syphilis manifestly contagious, and of these accidents, in an especial manner, the mucous postule or flat tubercle; 2. That this rule applies to the nurse and the child she suckles, as well as to others, and there is no reason to suppose that the product of these accidents has different characters in the child at the breast of one, what it has in the adult.

Narcotic Injections in Neuralgia.—Charles Hunter, Esq., House Surgeon to St. George's Hospital, at the close of a paper on this subject, in the Medical Times and Gazette, offers the following conclusions:

"In considering the results of the trial of the local treatment in the two cases, the advantages obtained appear to me to be—

"1. That much less constitutional (nervous) irritation attends the local introduction of the narcotic than when it is given by the stomach.

"2. That the effect of the narcotic is more immediately produced.

"3. The action of the narcotic appears more sure when injected. The exact amount taken into the circulation can be more readily seen, and the risk of contamination or alteration which it is exposed to, given by the stomach, is avoided.

"4. It appears to exert more benefit on the local affection when it has to be absorbed from the part affected itself, probably from being brought more directly into contact with the nerves involved in the disease.

"On the other hand, there are the disadvantages: these are, chiefly—

"1. The pain occasioned by the introduction of the fine canula.

"2. The chance of the fluid escaping from the wound or puncture.

"3. The production of local inflammation, effusion of blood, abscess."—*Dental Cosmos.*

Jaundice.—Dr. F. T. Frerichs, Professor in Breslin, has recently published his Clinical Treatise on Diseases of the Liver, in which he gives, as the result of his observations, that there are only two ways in which Jaundice can be produced—*first*, by an increased *absorption* of bile from the liver into the blood, as when the bile is obstructed in its passage to the intestines, or when the liver manufactures bile too rapidly; *secondly*, by too much bile being poured into the intestines, and thence absorbed into the blood faster than the biliary matter is disintegrated.

Bees.——Chloroform has been effectually used in removing honey from bee-hives in England and this country.

The vapor of chloroform is blown into the hive until the bees are asleep, when they can be handled with safety, and in a few hours they are again as lively and busy as ever.

Saline Injections.—M. Roche states that he has been so successful in some cases in which he has tried the injection of a solution of chloride of sodium into the throat, that in his next case he is disposed to employ it as the sole means of treatment. He practices, in fact, a continuous, or almost continuous irrigation of the throat, by means of Eguisier's irrigator, provided with a canula having a very small jet. He believes that it is in such irrigations, whether employing salt, alum, or the chlorates, we should seek for curative agents.

Anæsthetics.—M. Delabarre, in a communication to the Academy, states that he finds the safest and most agreeable way of administering ether or chloroform, or the two combined, is by a chibouk, having a double tube attached, by means of which any quantity of air desired may be admitted. The patient at first smokes air only until accustomed to the action, when the anæsthetic vapor is gradually admitted.—*Bull. de l'Acad.*

Ascarides.—Dr. Daniel Smith states that, during a practice of more than forty years, he has never known assafœtida and aloes to fail of an immediate cure. He has usually employed the tincture, sometimes clearing out the bowels first by a smart purgative.

Odontalgia.—Dr. R. W. Evans strongly recommends the muriate of ammonia in odontalgia. He says: "Take about two drachms, and apply it to the aching tooth for about half an hour, during which time a constant stream of saliva will flow from the mouth."

Glycerine was successfully used to prevent the pitted cicatrices from small-pox postules, during the recent epidemic at Berlin.

NORTH CAROLINA
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Original Communications and Translations.

A REVIEW OF DR. OTIS FREDERICK MANSON'S ESSAY ON "MALARIAL PNEUMONIA."

BY W. T. HOWARD, M. D., OF WARRENTON, N. C.

[*Presented to the State Medical Society of North Carolina May*, 1858.]

(CONTINUED.)

PART III.

IMMEDIATELY succeeding his account of the *physical signs*, Dr. Manson devotes a section to the "*Diagnosis between Malarial and other forms of Pneumonia*"; and then follows another section upon its "*Cause and Nature.*" We have already had occasion, while reviewing his description of the "*general course and progress*" of the disease, to comment, at length, upon the points of differential diagnosis, upon which he insists, between "malarial" and ordinary simple pneumonia. At present, we propose to canvass his opinions respecting the "*nature*"—the *pathological anatomy*—of "malarial pneumonia," and to offer a few remarks upon his views touching the diagnosis between it and *typhoid pneumonia.*

In the foregoing pages, we have had to allude, repeatedly, to Dr. Manson's statement, that "*malarial* pneumonia" is "*the* form of pneumonia *generally* seen by *Southern* observers"; and that although it is "often unrecognized," and "often confounded with simple, typhoid, and other varieties of pulmonic lesion," yet that it differs from them all so much, in fact, as to be

"*unlike* in character *and nature.*" It now remains to be seen upon what grounds these statements are based.

In order that Dr. Manson's views upon this highly important division of the subject may be fairly and connectedly set forth, we shall quote a few passages from the introduction to his account of the physical signs of the *second stage* of "malarial pneumonia," that have already been presented to the reader. The following, then, are the considerations adduced by Dr. Manson to sustain his positions in regard to the *pathological anatomy* of the disease under notice.

I.—"These terms (solidification, hepatization,) are synonymously used to denote that condition of the lung, in pneumonia, in which it is no longer permeable to the air, but doubtless are too often indiscriminately used. In this affection, as we have repeated, and shall endeavor to maintain, the state of the lung parenchyma partakes more of the character of congestion than active inflammation. There can be but little doubt but that, in a great proportion of cases, *some degree* of inflammation exists, as proven by the presence of fixed pain—the peculiar sputa and other symptoms; but often, as we have premised, the expectoration is of a different character, partaking more of the appearance of diluted blood, or of a sero-sanguineous nature.* When, therefore, perfect dullness on percussion, and absence of the respiratory sounds, betoken condensation of the lung in this disease, it is not, in my opinion, due to an inflammatory process with lymph effusion, as in the frank form of pneumonia, and to which the term hepatization has been properly applied. We shall endeavor to substantiate these views when the nature of this affection is considered." * *. *

"Andral considered hepatization to arise from congestion of blood, and not from lymph effusion, and in this opinion Stokes also concurs. Blakiston, we think, very reasonably concludes, and cites experiments in proof, that in pneumonia there are two kinds of solidification, one of which is produced by congestion, and the second by deposition of lymph."

Under the head above referred to, Dr. Manson gives the following "glance at the *pathological condition* of the lung."

"I have repeatedly stated, in the foregoing pages, my impressions in regard to the condition of the lung parenchyma in this affection, as partaking rather of the nature of active congestion than well-developed inflammation. This is proven, in my opinion, by the following circumstances: 1st, The sudden occurrence of solidification, as evinced by perfect flatness on percussion, and absence of the respiratory murmur. This sometimes takes place, in grave

* In this disease the expectoration is usually an exhalation, and not the result of inflammation.

cases, in a few hours after its onset, involving more than half of the entire lungs, accompanied by a cold skin, flagging pulse, and colliquative diarrhœa, a condition of system totally incompatible with the existence of the inflammatory process; 2dly, The disappearance of the physical signs is too rapid in many cases, to suppose that inflammation, terminating in plastic exudation, had produced this impermeable condition of the lungs.

"It is true, as we have stated, that there are some evidences of inflammation* mingled with the symptoms, and as active congestion is a state closely allied to inflammation, the latter may, and doubtless does occur, but it is evident that inflammation cannot be the general condition, as it is too expeditiously removed, in many cases of several days' duration, and by inadequate means, to render that view admissible."

1. If the terms "solidification" and "hepatization" are "*synonymously used* to denote that condition of the lung, in pneumonia, in which it is no longer permeable to the air;" if, in ordinary medical *parlance*, they are regarded as *synonymes*, and "*indiscriminately used*"; why discriminate in their use at all? Certainly not for the supposed reason that the latter term indicates "an inflammatory process with lymph effusion," although the solidified lung in the second stage of pneumonia is doubtless due to the air-cells being blocked up by lymph, rendering them impervious to air. The term *hepatization* was first employed, Lænnec tells us, by *Lælius a Fùnte*, and, as its derivation implies, indicates simply that the lung has become converted into a *liver-like* substance; and by Lænnec it was deemed as sufficiently correct to designate the condition of the lung in the second stage of pneumonia, because, says he, the lung has then acquired a consistence and weight altogether resembling those of the liver (*a*). Andral proposed *red softening* as a better term, on account of the great friability of the lung in this stage, and as establishing an important distinction between its parenchyma attacked with inflammation, and that of the liver, which is thus soft and friable only in certain pathological states. (*b*) Gerhard says the lung may be called *hardening*, on account of the increased consistency which is perceived when it is slightly pressed; *softening*, on account of the facility with which it is broken, if

* In Lænnec's cases, he only mentions "that *traces* of inflammation" were found after death.

(*a*) Op. cit., 209.

(*b*) Med. Clinic, vol. ii. 176,

the pressure be increased; *hepatization*, from its resemblance to the tissue of the liver; and that, in the case of children, this resemblance is so close, that he has known a bystander mistake a piece of lung for liver, although both tissues were before him. (*a*)

Now, until Dr. Manson has made *autopsies* in "malarial pneumonia," and has accurately ascertained the color and consistence of the lung parenchyma in its second stage, with what reason can he object to the term "*hepatization*"? For aught he knows to the contrary, it may be as good as any other to denote "condensation of the lung in this disease." At all events, his criticism upon the term does not seem, at present, to have been happily timed.

In this connection (see October number of this Journal, p. 28), Dr. Manson quotes several passages from Walshe respecting the conducting power of lungs in the stage of hepatization, and in respect to the fact that different specimens of hepatized tissue which the naked eye would judge to be in the same condition in regard to their shares of air, fluid, and semi-plastic substance, are acoustically in different physical states; but we have not been able to perceive what relation all this has to the question, whether the "condensation of the lung," in the second stage of the so-called "malarial pneumonia," be due to exudation of lymph, or to congestion of blood.*

2. The character of the *sputa*.—In proof of his allegation that, in "malarial pneumonia," the state of the lung parenchyma

(*a*) Op. cit., 136.

* Although the quotation from Walshe is introduced by Dr. Manson in the connection referred to, yet it may be that we have mistaken the application intended by him, as the passages in question immediately precede Dr. Manson's account of the *physical signs* of the second stage of "malarial pneumonia." But even if this be so; if the passages quoted were designed to have some special reference to the succeeding description of the physical signs, the reader is left by Dr. Manson to make the application for himself. And should the latter have attempted it, we strongly suspect that he would have been foiled; for if, as Walshe states may be the case, varying homogeneousness of different specimens of solidified lung be the key to the difficulty involved in the fact that different lungs, which the naked eye would judge to be in the same state physically, in regard of their shares of air, fluid, and semi-plastic matter, are, *acoustically*, in different physical states; yet, as he very justly adds, "Even if so, it is a *key which cannot practically be used.*"

partakes more of congestion than inflammation, and that the latter "cannot be the general condition," Dr. Manson brings forward the character of the *expectoration*, which, in this disease, says he, "is *usually* an *exhalation*, and *not* the result of *inflammation*." But we are met on the very threshhold with the counter-statement, that the "*peculiar sputa*," which (in connection with other circumstances) prove that "some degree of *inflammation* exists," occur "*in a great proportion of cases*"! If this be so, how can the expectoration, which is said to be simply "an *exhalation*, and *not* the result of inflammation," be "*usually*" present—the *type-form* of the sputum—in "malarial pneumonia"? Let us look at the matter, however, in both aspects.

In those instances in which it is admitted that "some degree of *inflammation* exists," and which comprise "*a great proportion*" of the cases, to what extent does this inflammation proceed? Does it remain simply in the *first stage?* If so, then, in "*malarial* pneumonia," the inflammation not passing beyond the *first stage*, there can be no other stages to the disease. And yet Dr. Manson says there are *three stages*, although the *third* is "very rare." Well, the inflammation advancing on to the *second stage*, what becomes of its products? The "condensation of the lung in this disease," says Dr. Manson, is "not due to an inflammatory process with lymph effusion." But what evidence is there of that? In his description of the "general course and progress" of the disease, Dr. Manson states that "pain in the side, of the *pleuritic* character," is "absent in very few instances." Does "pain in the side, of the *pleuritic character*," when "ushered in by a chill," and "soon succeeded" by "reactive fever, dyspnœa, and cough," indicate an inflammation of the pleura,—*pleurisy?* Such, certainly, are its rational symptoms. And if the state of the forces admit of *pleurisy*, does the poison of malaria, which tinges its usual phenomena, so modify the inflammatory process as to prevent the effusion of lymph—the characteristic primary product of this process? If not, does the pleura which escapes being inflamed in only a very few instances of "malarial pneumonia,"—the inflammation being circumscribed and comparatively unimportant,—yield the usual product of in-

flammation, and the pulmonary parenchyma itself—which sustains the chief shock of the disease—yield no such product? Or, are we to understand that when pleurisy and pneumonia are imbued by a malarious influence, we do not have the usual products of inflammation? And is this the case with inflammations of all other structures when they happen to be impressed by the poison of malaria? Or, finally, are *pleurisy* and *pneumonia* exceptions to the rule? Really it seems to us that, against a doctrine which leads to such consequences as these questions pre-suppose, there is, antecedent to all inquiry, a reasonable and strong presumption. The mind instinctively calls aloud for *proof*, for none whatever is to be found in the Essay under review.

We will now consider, on the other hand, the expectoration which presents the "appearance of diluted blood, or of a serosanguineous nature," and which is said to be merely "an *exhalation*, and *not* the result of inflammation." Again the inquiry immediately suggests itself—what evidence is there of the correctness of this assertion? For it stands unsupported by any kind of proof either of fact or argument. The postulate assumes the validity of two propositions; first, that the sputum described is "*usually*" witnessed in "malarial pneumonia"; and secondly, that, when present, it is due to a process of "*exhalation*," and is *not* the product of "*inflammation;*" both of which, it need scarcely be said, must be demonstrated to be true, before they can avail Dr. Manson anything in the position they are adduced to sustain. That the expectoration in question is not confined to what Dr. Manson terms "*malarial* pneumonia," we have already proved by the authority and experience of Professor Wood, who regards it as presenting a very striking peculiarity of *typhoid pneumonia*, which, according to *Dr. Manson*, differs from the former so much as to be "unlike in character and *nature*." That this variety of the expectoration is met with in certain cases of pneumonia, chiefly of an asthenic type, whether offering the tokens of a malarial or periodic fever element or not, cannot, we presume, be questioned by any one of much experience; but we think Dr. Manson greatly errs in supposing that it

"*usually*" occurs in "*malarial* pneumonia," or, indeed, in any form of pneumonia which generally prevails in this section. Thus, of thirty-two cases of pneumonia that have occurred in our practice (exclusive of children) during the past two seasons, it was observed in only *two*. Indeed, as already remarked, Dr. Manson contradicts and nullifies (as it appears to us) his own statement in advance, when he admits that the "*peculiar sputa*" which result from "some degree of *inflammation*," exist "in a *great proportion* of cases"; and, thereby, confesses that the expectoration under notice—which is represented as "*not* the result of *inflammation*"—can not be the predominant form.

But, with what reason does Dr. Manson allege that this variety of the expectoration is simply "an *exhalation*, and *not* the result of inflammation"? Is it not well known at the present time, that, strictly speaking, the fancied open-mouthed vessels, described by Bichat as *exhalants*, have no existence; that none, at Dr. Gross remarks, have ever been demonstrated? When fluids are found in unnatural positions, they must have passed out by rupture of the coats of the vessels that contained them, or by a process of "exosmose, transudation, oozing, or diapedesis," since all animal tissues are permeable to fluids. In certain diseases, as scurvy and typhus fever, it is thought by some that, owing to a special deviation of the blood from the normal standard, this fluid percolates through the relaxed parieties of the minute vessels, carrying with it its different component elements, and giving rise to those hemorrhagic effusions that are not infrequent in those affections; but since microscopists have never been able to detect pores in the capillaries large enough to admit the passage of blood corpuscles, or, indeed, any pores at all, others affirm that blood must always escape by rupture of the vessels. Be this, however, as it may, still there is no satisfactory evidence that the "sero-sanguineous" expectoration, of which Dr. Manson speaks, is the result simply of exosmose or diapedesis; and there is no apparent reason why it is not the product of an "inflammatory process." It should be remembered that serum itself is one of the products of inflammation; and it is well understood that lymph is effused under a variety of forms,

and in its physical properties is liable, like other morbid products, to be modified by adventitious circumstances, and hence that it does not always exhibit the same appearances. (*a*) Moreover, as Paget states, that which is usually regarded as a serous effusion in inflammation, is, in many instances, a fluid that contains fibrine, and resembles the *liquor sanguinis* rather than mere serum. And, on the other hand, as Dr. Gross remarks, when lymph is squeezed, it yields a small quantity of fluid, "*identical with the serum of the blood.*" In pneumonia, the exudation receives its characteristic rusty tinge by being mixed with the extravasated blood-corpuscles that are poured from the ruptured vessels of the inflamed lobules, or, in some instances, the coloring matter of the corpuscles may be dissolved in the effusion, giving it the appearance of blood.

II. We will now pass on to Dr. Manson's "glance at the *pathological condition* of the lung" in "malarial pneumonia." That the "lung parenchyma in this affection," says he, "partakes rather of the nature of active congestion than well-developed inflammation, is proven by the following circumstances":

"1st, The sudden occurrence of solidification, as evinced by perfect flatness on percussion, and absence of the respiratory murmur. This sometimes takes place, in grave cases, in a few hours after its onset, involving more than half of the entire lungs, accompanied by a cold skin, flagging pulse, and colliquative diarrhœa, a condition of system totally incompatible with the existence of the inflammatory process; 2dly, The disappearance of the physical signs is too rapid, in many cases, to suppose that inflammation, terminating in plastic exudation, had produced this impermeable condition of the lungs."

Upon reading the above quotation, one naturally asks—1st, What is the space of time meant to be embraced in the phrase, "*in a few hours after its onset?*" and, 2dly, if, sometimes, in *grave* cases, "*more than half of the entire lungs become solid in a few hours after the onset,*" and, then, this solidity disappears too rapidly to suppose that it was produced by inflammation terminating in plastic exudation, does Dr. Manson desire to be understood as affirming that this concurrence of phenomena is

(*a*) Gross' Path. Anat., 63, 64.

usually witnessed in that form of pneumonia which is *generally* met with in *this* section? It seems to us that such an inference is scarcely warranted by his language; and yet, unless this be the case, why apply the alleged occurrence of a certain train of phenomena in confessedly a rare and special class of cases, to the solution of another, immensely larger, and different class of cases, in which the phenomena in question do not occur?

But let us look into these points, and endeavor to determine whether Dr. Manson's conclusions in the premises be warranted by sound pathology.

1. According to Dr. Manson—

"The sudden occurrence of solidification" in pneumonia is "incompatible with the existence of the inflammatory process."

The archetype of this idea, if we mistake not, is found in the treatise of Stokes on Diseases of the Chest. That physician, having observed some examples of a secondary disease of the lung, supervening on typhus fever, or that condition of the system in which diffuse inflammations are liable to ensue, characterized by the occurrence of *solidity*, and its disappearance, in the course of twenty-four hours, inferred that, in acute sthenic pneumonia also, the solidity arises not from any deposition of lymph, but merely from an excessive congestion of blood. (*a*). He does not profess, however, to have based his views upon any anatomical proofs of their correctness, but merely upon the treacherous hypotheses of clinical inference; and the reader need not be surprised, therefore, to learn that the occurrence of the same phenomena which induced Dr. Stokes to refer the solidity of *acute sthenic pneumonia* to an excessive congestion of blood, and not to any deposition of lymph, induced Dr. Manson to make the same inference in regard to the solidity of "*malarial pneumonia*," although that of the former, as he candidly admits, *is* "due to an inflammatory process with lymph effusion," while that of the latter, he says, is not. A very good example this, of the very uncertain value of mere clinical inferences, and of the opposite conclusions to which they often lead intelligent observers, when substituted for the positive teachings of pathological

(*a*) Op. cit., 267.

anatomy. It certainly does not seem very consistent in the eminent Dublin Doctor to have stated that "the most frequent termination of the typhoid solidity," of which he speaks, is in the stage of "*interstitial suppuration*"; and that "when we reflect on the circumstances of the disease, such a termination seems *easily intelligible*"; and yet, at the same time, to have referred its occurrence simply to "an excessive congestion of blood." That Dr. Stokes is wrong in attributing the solidity of ordinary frank pneumonia to "an excessive congestion of blood," instead of "an inflammatory process with lymph effusion," we have the authority of Dr. Manson himself. Let us see, then, whether this form of pneumonia does not, likewise, frequently pass into the second stage in no very lengthened period; in some instances, indeed, in the course of a "few hours."

In speaking of ordinary acute sthenic pneumonia, Swett states, that it "passes *rapidly into the second stage;* so rapidly, in fact, that it is rare not to find it in cases which have exhibited any symptoms of the disease during life." (*a*) Flint says that he has known bronchial respiration to take the place of the vesicular murmur in simple frank pneumonitis in the space of *twenty-four hours*; (*b*) and we ourselves have detected it in *twenty hours* from the onset. After referring to certain cases, usually of short duration, in which the inflammation never reaches the second degree, owing to the speedy termination in resolution; and to some, and particularly those in which a great portion of one, or, that which is extremely rare, the whole of the two lungs, is invaded, and that terminate fatally without having attained the second stage; Chomel adds—"But more commonly pneumonia continues its march; it passes to the state of *hepatization*, and this transition is often very rapid (*et cette transition est souvent très rapide*)." (*c*) Grisolle, also, states that the passage from the first to the second stage takes place very rapidly (*se fait très rapidement*); that he has ascertained the existence of bronchial respiration in an individual in whom the beginning of pneumonia was traced back scarcely twelve hours; and that, on the third

(*a*) Diseases of the Chest, 67. (*b*) Physical Exploration, &c., 412.
(*c*) Dict. de Méd., t. xxv. 172.

day of the affection, one finds the crepitant râle mixed with the *souffle tubaire*, in *two-thirds of the cases.*(*a*) Under the caption, "*Duration of pneumonia, and of its different stages,*" Lænnec states:

"Acute pneumonia is one of those diseases which, from the rapidity and brevity of their course, and the shortness of the period in which treatment can be beneficially applied, demand the utmost attention and vigilance on the part of the physician. Its general duration, however, as well as that of each of its stages, is variable. I have several times seen the engorgement (or first degree) continue for seven or eight days, and affect the whole lung and part of the other, and prove fatal before the occurrence of any very distinct hepatization. * * * In other cases, on the contrary, particularly when the disease has attacked debilitated or very old subjects, or supervened in the course of another severe malady, the inflammation reaches the stages of *purulent infiltration in the short space of thirty-six or even twenty-four hours.* With the exceptions just stated, I think we may fix the duration of the different stages of pneumonia as follows: the obstruction, or first stage, usually lasts from twelve hours to three days, before passing to the state of complete hepatization; this lasts from one to three days before spots of purulent infiltration make their appearance; and the period of suppuration (from the time when the concrete purulent infiltration is distinctly perceptible, until this is completely softened to a viscid fluid) varies from two to six days. (*b*)

Here we find it stated by the illustrious Lænnec, "great in pathology as in diagnosis,"—that, under certain circumstances, pneumonia reaches the "*stages of purulent infiltration in the short space of thirty-six or even twenty-four hours;*" but that the "first stage *usually* lasts from *twelve hours* to three days, before passing into the state of *complete hepatization.*" It is impossible to doubt that the solidity of a lung is "*due to an inflammatory process,*" when it issues in the formation of pus, whether the period of its existence be long or short. "There really exists pneumonia," says Professor Trousseau, in a clinical lecture at Hotel Dieu, Paris, on the difficulties of Diagnosis in certain cases of this disease,—"which is essentially malign and suppurative; in effect, such a pneumonia does not follow in any degree the ordinary course, but enters in a trice the suppurative stage"; and he fortifies his statement by previously remarking on the case that elicited it—"His lungs are now before you, and they ex-

(*a*) Traité de Pneumonie, 303. (*b*) Op. cit., 219, 220.

hibit a pneumonia of the *third stage*." (*a*) In the work of Doctor Blakiston on Diseases of the Chest, to which Dr. Manson repeatedly refers, we find recorded two cases of pneumonia, one of the sthenic, and the other of the asthenic or typhoid type, in both of which the solidity of the lung was due to "an inflammatory process" with a rapid "lymph effusion." The former, a draper's assistant, ætat. 21, previously in good health, was seized after exposure to cold, and, when seen on the fourth day, was found to be laboring under pneumonia of the lower half of the *right* lung, as evidenced by the existence of crepitant rattle and dullness on percussion of the corresponding part of the chest, and traces of rusty-colored expectoration. On the seventh day, the upper half of the *left* lung, which was in a healthy state on the previous day, was found dull on percussion, and coarse blowing respiratory sound was heard over it. On the *fifth* day subsequently, he died. The *upper* lobe of the *right* lung, otherwise healthy, contained two or three small masses of red induration, and contained some few small patches, of a grayish yellow color, the middle lobe was in a state of red induration, while the whole of the lower lobe was one mass of solidification. The upper lobe of the *left* lung was in a *similar state*. In commenting on this case, Doctor Blakiston observes: "We are forced to admit that *lymph was very speedily formed* without much fluid. * * * * * The suddenness with which solidification of the *left* lung occurred forcibly reminded me of what often takes place during the course of typhus fever, which has

(*a*) Virginia Med. Jour., vol. x. 487, 488.—The case in question died on the ninth day of his disease; but, on the fifth, a careful stethoscopic examination by the most accomplished practitioner in the city of Paris, could only reveal "on the side of the shoulder at the extreme end of the clavicle, *in a circumscribed point not larger than a franc piece*, a little blowing, and a very slight crepitant rôle; in the axilla there was nothing heard, nor in the lesser scapular region, nor in any other part of the chest;" but "on that evening, even the blowing occupied all the sub-clavicular region; the next day all the inferior portion, and on the fourth day he died"—the ninth of his illness, as he entered the ward of St. Agnes on the fifth day of his attack. This case shows, in a very striking manner, how rapidly exudation matter is poured out in certain states of the system; for it is evident that a large portion of the pulmonary parenchyma passed into the *third stage* even, on the fourth day after it was invaded by the "inflammatory process."

been mentioned by Dr. Stokes and others, and must have been observed by all persons who have witnessed many cases of fever. Dr. Stokes argues that the suddenness with which solidification thus takes place is a proof that it arises from congestion alone; but here was an equally rapid solidification, and on dissection this dry, gray induration was found, and was *evidently produced by the deposit of lymph.* * * In these sudden solidifications in typhus, * * the appearance of the diseased parts will be found, as a general rule, to present a more highly granulated surface than in sthenic pneumonia; and although this may partly arise from the dark color of the blood contrasting with the walls of the vesicles, yet it cannot altogether be thus produced." After giving the *post mortem* appearances of the lungs in the case of typhoid pneumonia above adverted to,—that of a traveller in the beer trade, ætat. 39, formerly intemperate,—he adds: "Here also there appears to have been *a rapid deposit of dry lymph* in the lungs of a person in a typhoid state. The ordinary characters of acute pneumonia were altogether absent". (*a*) Finally, Dr. John P. Little, of Richmond, Virginia, reports a case of rapid hepatization, as follows:

"B——, a negro slave, working in a factory, had enjoyed good health ever since his recovery from an attack of typhoid fever which occurred twenty-one months previously. He had been regularly at work until seven o'clock, A. M., of the day of his attack; at this time he was taken with a chill. He had had slight cough for a day or two. I saw him at five o'clock, P. M. He was then in the second stage of pneumonia, the greater part of the right lung being hepatized. He died at two o'clock the following day.

"*Autopsy.*—The right lung was entirely solidified; there was no appearance of pus; the left lung was healthy; the costal pleuræ were not involved in the disease; no tubercles could be detected. The brain, the heart, the stomach and the liver, presented no abnormal appearance." (*b*)

Now, here is a case of hepatization in *nine hours* from the time when the patient was seized with a chill, while he was regularly at his work in a factory, in the enjoyment of good health, only a slight cough having existed for a day or two.*

(*a*) Op. cit., 287–290. (*b*) Stethoscope, vol. iii. 597, 598.

* It is but fair to add, that Dr. Little expresses the *opinion*, that "this patient must have had latent pneumonia for some time," as, says he, "it is impossible

In view, then, of the considerations now brought forward; the fact that, in acute frank pneumonia—in which the solidity of the lung is admitted by Doctor Manson to be owing to the deposit of lymph—the inflammation, according to the best observers, often passes into the second stage very rapidly, as is proved alike by the physical signs during life, and the test of examinations made after death; and that, in some rare instances, even the stage of *purulent infiltration* is reached in a period varying from twenty-four hours to three or four days; and that in typhoid pneumonia, also, the solidity may be produced by a rapid deposit of lymph, presenting a highly granulated surface; the argument based upon "the sudden occurrence of solidification" in certain cases of "malarial pneumonia," as showing that in those cases this state is, of necessity, "not due to an inflammatory process with lymph effusion," does not appear to bear the scrutiny either of actual clinical observation, nor of an advanced pathology. Indeed, what reason, *à priori*, is there for supposing that "the sudden occurrence of solidification," in the cases in question, necessarily excludes the idea that it cannot be caused by inflammatory exudation? Is it not now well known that lymph is often very speedily formed? Prof. Wood states that—"Coagulable lymph is often exuded *immediately* after inflammation has been established." (*a*) Professor Gross affirms—"The *period* at which the deposition of lymph com-

to believe that nine hours could have been the space of time between perfect health and the second stage of inflammation of the lung." We accept the Doctor's *facts;* but, with all suitable deference, we beg to demur to his conclusions. They are not warranted (as it seems to us) by his own report of the case; there is no apparent evidence whatever to sustain them; on the contrary, they are directly opposed to the evidence. It is much more in accordance with our knowledge of the history of pneumonia to believe, that the greater part of a lung should become hepatized in nine hours, than that a man should be laboring regularly at his work in a factory, in the enjoyment of "perfect health," and yet be the subject of a "latent pneumonia for *some time.*" A *chill*, be it remembered, usually heralds the onset of pneumonia; as Trousseau says, "this symptom is rarely absent in a pneumonia, and it is *sufficient of itself in the largest number of cases to lead us to investigate for a pneumonia.*" In the case under notice, the patient was suddenly seized with a *chill*, in the full tide of health, while being regularly engaged at his work in a factory; and it is well known that few diseases more suddenly prostrate the strength than pneumonia.

(*a*) Prac. of Med., vol. i. 29. Fourth edit.

mences is influenced by different circumstances, the principal of which are referable to the intensity of the inflammation and the nature of the affected structure, tissue, or organ. If an incision be made into the skin of the finger, its surface will become covered with *plastic matter* within less than an hour after it has ceased to bleed; and the flaps of the stump in amputation often become *thoroughly glaced with lymph* nearly in the same time. Some years ago, I had occasion to operate upon a gentleman on account of internal strangulation of the bowel. Some adhesions were found between the intestinal convolutions, but they were slight. The man died precisely four hours after the operation; and on laying open the abdomen shortly afterwards, I discovered a thin film of lymph spread over nearly the whole of the small bowel, and also over a large part of the parietal portion of the peritoneum. In a young man who died under my care of a gunshot wound, which penetrated the liver and diaphragm, I found both the pleura and peritoneum extensively coated with this substance, nine hours after the reception of the injury. *In many cases there is reason to believe that the effusion takes place almost at the very onset of the inflammation, and that it proceeds, with greater or less rapidity*, until the disease either abates, passes into suppuration, assumes a chronic form, or destroys life."(*a*). And, quite recently, (July 16th), at the Pennsylvania Hospital,—after amputation of the leg on account of a railway injury,—Prof. Pancoast directed that—"the stump was to be left exposed to the air until all disposition to bleed had disappeared, and until it was polished over with a *plasma of lymph*, in accordance with a method long practiced in this house"(*b*); and it is well known that, after this operation, the leg is dressed within an hour—certainly before the lungs could become *solidified*, even in *Dr. Manson's* "malarial pneumonia."

Can it be necessary to add anything further on this head?

In regard to those "*grave cases*" of "malarial pneumonia"—confessedly a rare and special class—in which *more than half of the entire lungs* are said to become *suddenly solidified in a few hours after the onset, accompanied by a cold skin, flagging pulse,*

(*a*) Op. cit., 66, 67. (*b*) Med. and Surg. Reporter, vol. ii., N. S., 362.

and diarrhœa, they are but examples of the pernicious form of periodic fever, occurring coincidently with an attack of pneumonia. Although rarely met with in this latitude; very rarely, we should say, since, in a practice of fifteen years, we have never seen a case; yet there is no reason—*à priori*—why they may not "sometimes" occur in miasmatic districts, as local inflammations of various grades and types may be developed in connection with every form of periodic fever. In the immense majority of such cases, however, it is the ordinary forms of remittent and intermittent fevers with which pneumonia is associated. The cold skin and flagging pulse—symptoms here of nervous prostration—which indicate the presence of the pernicious variety of the fever in question, are due neither to inflammation nor to congestion, but are dependent, as Dr. Wood has pointed out, upon the peculiar state of the innervation.

2. Dr. Manson urges further that—

"The disappearance of the physical signs is too rapid in many cases to suppose that inflammation, terminating in plastic exudation, had produced this impermeable condition of the lungs."

Here, again, we are at a loss, *in limine*, to know what is the precise space of time to which Dr. Manson refers, when he says that, in "malarial pneumonia", the disappearance of the physical signs is too rapid in many cases to render it supposable that the impermeability of the lungs was produced by inflammatory exudation. But supposing, in the absence of exact information, that both authors, in using the same word, had in view about the same period, we would simply remark that the experience of Stokes amputates, at a single cut, this limb of Dr. Manson's argument; for Stokes states positively, "that we may observe the *rapid resolution* of the disease in the *primary and sthenic cases*," in which (as we have seen) Dr. Manson admits that the impermeability of the lungs *is* due to the exudation of lymph. Now this is a well-attested *fact:* one that is based upon the evidence of an accurate observer of clinical phenomena, and is not a mere clinical *inference* respecting the nature of a pathological lesion. Moreover, Stokes' experience is sustained by others.

Thus, Grisolle says (in speaking of ordinary frank pneumonia) that he has seen three cases that, at the morning visit, presented to auscultation *un souffle tubaire pur*, in which, in the evening, the bronchial respiration was already mingled with the *crepitans redux*, which ceased to be perceptible in the visit of the next morning, even after coughing. (P. 308). In referring to Stokes' agreeing with Andral, in the supposition that the solidification of pneumonia arises, not from any deposition of lymph, but merely from excessive congestion of blood, Copeland remarks: "In proof of this, he argues that the rapid appearance of this stage, and the rapid subsidence of the symptoms attending it, are evidences of no farther change than congestion. But this is to assume that excessive congestion is attended by the same phenomena as accompany infiltration of lymph; and these occurrences are no disproofs even of effusion of lymph having occurred; for it is *well known that absorption in the lungs is sometimes* REMARKABLY RAPID." (*a*) And Professor Austin Flint, in a clinical report on fifteen cases of pneumonia, observed by him at the New Orleans Charity Hospital, affirms that "Convalescence was *rapid in all instances*, save one in which the pneumonia occurred as a complication of typhoid fever. Exclusive of this instance, the resolution of the affected lung, as shown by the *disappearance of the physical signs denoting solidification, was rapid.*" (A. J. M. Science, N. S., vol. xxxviii. 283). The rapidity with which pneumonia may be resolved is occasionally observed, in a striking manner, by the incidental occurrence of other diseases. Thus, Andral reports the case of a man affected with double pneumonia, characterized by full and frequent pulse, hot skin, red and viscid sputa, high and hurried respiration, anxious countenance, impaired resonance on percussion, and well-marked general crepitant rhonchus. A fatal prognosis was already forming, when, on the evening of the fourth day, the eruption of small-pox appeared on the face, chest and arms, and the inflammation of the lungs "disappeared as if by enchantment." (*b*) Graves saw a case of pneumonia advanced to the *second stage*, in which all the morbid pneumonia referable to

(*a*) Med. Dictionary, vol. ii. 882. (*b*) Op. cit., 168, 169.

the chest, disappeared entirely in only *thirty-six hours*, by the supervention of a grave attack of Indian cholera. (*a*) And Boling, in speaking of a rarely observed, but fatal effect of the *tartrate of antimony and potassa* on the gastro-intestinal canal in pneumonia, states: "A more or less rapid disappearance of the signs and symptoms of the primary disease takes place. From a state of almost complete solidification of an entire lung, with dullness on percussion, and *bronchial respiration*, in the course of *four or five* hours, I have found the pulmonary tissue permeable, and the chest resonant, and yielding a healthy respiratory murmur; a corresponding improvement in the cough, thoracic pain, difficulty of breathing, &c., proceeding at an equal rate. The rapidity with which this change in the condition of the lung takes place, is proportionate to the violence and rapidity of the newly-developed abdominal disease."(*b*)

It is evident, therefore, from these facts and considerations, that Dr. Manson is not sustained in his conclusion that the impermeability of the lung, in certain cases of "malarial pneumonia," is not due to inflammatory exudation, because, in those cases, the physical signs may disappear in a shorter space of time than we are wont habitually to observe in ordinary simple pneumonia.

Dr. Manson insists, however, that there certainly are cases of pneumonia in which solidification of the lung is caused simply by *congestion of blood;* and, in proof of the allegation, he states that—

III. "Andral* considered hepatization to arise from congestion of blood, and not from lymph effusion, and in this opinion Stokes also concurs. Blakiston,† we think, very reasonably concludes, and cites experiments in proof, that in pneumonia there are two forms of solidification, one of which is produced by congestion, and the second by deposition of lymph."

If it be really true, that there is a form of *pneumonia* in which solidification arises merely from an intense congestion of blood, still it would rest with Dr. Manson to show that the so-termed "*malarial* pneumonia" belongs to the class in question. We

(*a*) Grisolle, op. cit., 317. (*b*) Amer. Jour. Med. Sci., N. S., vol. xxii. 331.

* Path. Anat., vol. i., 511. † Diseases of the Chest, 286.

have just seen how far short the evidence, already advanced by him, comes of sustaining his positions. We will now briefly consider what importance should be properly attached to the testimony of Andral and Blakiston, adduced by Dr. Manson, as to whether such a form of *pneumonia* exists at all.

1. *Andral's Testimony.*—We have not seen Andral's Pathological Anatomy, to which Dr. Manson refers in the above quotation. Blakiston, however, (who indicates the same volume and page of the work to which Dr. Manson makes reference), and others, represent Andral as maintaining broadly, and without reservation, that the solidity of *ordinary acute sthenic pneumonia* arises not from any deposition of lymph in the air-cells and tubes, but merely from an excessive congestion of blood in the vessels; an opinion confessedly erroneous, according to Dr. Manson himself. Moreover, Andral does not seem to have held this view in later years; for, in his Medical Clinic (a more recent work, if we mistake not), he states—"Pneumonia consists essentially in *inflammation of the pulmonary vesicles*, the inner surface of which secretes a liquid at first muco-sanguinolent, and then purulent; according as the inflammation proceeds, the liquid secreted becomes thicker and more viscid; it can no longer be expelled from the cavity where it is contained; it accumulates there, obstructs and distends it, and *gives rise to those numerous granulations, which appear peculiarly to constitute the red hepatization of the pulmonary tissue.*" (*a*) Does this language comport with the statement that *hepatization* arises from *congestion of blood?*

2. *Blakiston's Testimony.*—This author has little to say of the matter in hand. While stating that Reynaud has demonstrated that, in ordinary acute pneumonia, lymph is deposited in the interior of the air-vesicles and minute air-tubes, forming cylinders, which, when detached and drawn out, present mouldings of their vesicular terminations, he admits, at the same time, upon the examination of dried specimens of hepatized lung in the manner recommended by Andral in his Pathological Anatomy (the "*experiments*" to which Dr. Manson refers), that, in

(*a*) Med. Clinic, vol. ii. 178.

"*some* cases, hepatization arises *chiefly* from coagulation of blood in the vessels;" thus making out two forms of ordinary acute pneumonia, one of which is produced by congestion and inflammatory swelling of the vessels, and the other by the deposition of lymph; still, he appears to be in doubt about it, for he proceeds to state, on the same page, that the different "appearance of the surface of an incised portion of hepatized lung may arise from a difference in the *character of the exudation* rather than from the amount of congestion, as supposed by Andral"; and finally adds—"An extensive series of observations can alone settle the point, which is not of any great moment."(*a*) It is worthy of remark, that Blakiston has furnished no example of hepatization arising merely from congestion of blood, in his list of cases illustrative of the views advanced in his text; and that all of his cases were instances of pneumonia, in which there was an exudation of plastic matter. The examination of dried specimens of hepatized lung ought to have but small weight, as it seems to us, in determining this question, when contrasted with the now well-established fact of the positive presence of lymph in the air-cells, blocking up, and finally completely obliterating their cavities.

As we have previously remarked, inflammatory lymph is exuded under a variety of forms. The conditions which are chiefly powerful in determining its character and tendency are, according to Paget—1, The state of the blood; 2, The seat of the inflammation; and, 3, The degree of the inflammation. According to this pathologist, there are two chief forms of inflammatory lymph, which, though often seen mixed, in the same material, are so distinct as to warrant the reckoning of two varieties, distinguished by the names of *fibrinous* and *corpuscular*.

The *fibrinous* variety—the *euplastic* of Williams (*b*)—embraces those instances in which it is effused as a liquid, which coagulates into the solid form, and yields, when the fluid is pressed out, either an opaque-whitish, elastic substance, having the general properties of the fibrine of the clot of blood, or the softer

(*a*) Diseases of the Chest, 286, 287.
(*b*) Principles of Medicine, *fourth* Amer. edit., 249.

and less-developed fibrine of the chyle or the absorbed lymph. It occurs in acute inflammations of healthy and vigorous subjects, and microscopically is distinguished by the small proportion of exudation-corpuscles.

In the *corpuscular* variety of inflammatory lymph, no coagulation takes place; but corpuscles form and float free in the liquid part. Typical examples of this variety are found in blisters raised in cachectic persons, &c., and in *some instances of pneumonia.* Little healthy fibrine is observed, and exudation-corpuscles are in great abundance. This variety characterizes the inflammations of asthenic states of the system.

Between these two typical forms of inflammatory lymph, there are many intermediate stages; and in a large number of instances, the fibrine and the corpuscles mingle together, mixed in various proportions, forming an effusion of a compound character. (*a*)

It is obvious, then, that there may be cases of pneumonia in which the lymph effused may present any of the forms now described, and that we ought not to look for uniformity in the character of the exudation. "Lymph effused into the substance of the lung," says Barlow, "may be either of the fibrinous or corpuscular kind, and it may vary as regards the proportion between its different constituents. And there is good reason for believing that effusion of serum containing but a small proportion of fibrine or corpuscles sometimes takes place *suddenly into the cells,* as a consequence of inflammatory engorgement of the lungs, and in the place, if we may be allowed the expression, of the plastic fibrinous lymph which constitutes hepatization." (*b*) We have occasionally seen examples of pneumonia in this section, both with, and without, the tokens of a malarious influence, in which we have suspected that the effusion was of the character described by Barlow; and it is highly probable that such was the case in those instances of "malarial pneumonia," referred to by Dr. Manson, in which the signs of solidification were speedily observed after the onset, and that presented the so-called sero-sanguineous expectoration. Doctor Manson's error

(*a*) Lectures on Surgical Pathology, by James Paget, F.R.S., 216-218.
(*b*) Manual of the Practice of Medicine, 191.

consists, as it appears to us, in his regarding these cases as very frequent, and constituting the *type-form* of "*malarial* pneumonia"; in his attributing the phenomena offered by them to an intense congestion, rather than to an inflammatory process, as if the impermeable condition of the lungs in pneumonia must, of necessity, be produced by "*plastic* exudation." No one, we presume, will maintain that, in different states of the system, and in different sections of this widely-extended country, the exudation in pneumonia always presents the same character; yet it is doubtless equally true that exudation occurs in all cases in some one of its forms. If this be not so, then there can be no *pneumonia* in the cases in question, and it is a complete misnomer to apply to them that title. Indeed, in speaking of the second stage of "malarial pneumonia," Dr. Manson admits, "that in a great proportion of cases, *some degree* of inflammation exists"; but what are we to understand by the statement that "*some degree* of inflammation exists" in the *second degree* (second stage) of the disease; and that in the *first degree* (first stage), there "is doubtless an intense congestion of the parenchyma, associated, perhaps, in *some degree*, with inflammation"? There is "*some degree*" of "inflammation," says Dr. Manson, in the *first degree*, and "*some degree* of inflammation," also, in the *second degree*, of "*malarial* pneumonia",—the same words being used, in both instances, when speaking of different degrees (stages) of the same disease, without any effort being made to define their meaning! We must confess, that we do not apprehend the import of this language. To us it is unintelligible. We mean to treat the talented author with all respect; but it seems to us that, while he is not willing to be understood as entirely excluding inflammation from playing any part in the pathology of the affection under notice, yet (judging from the Essay before us), it would appear that he is fairly exposed to the suspicion of not having, himself, a very clear and accurate idea of what that part is.

IV. We will now enquire whether *post mortem* examinations throw any light on the subject in hand. As we have already

stated in the preceding pages, Dr. Manson gives us no information of this character as the result of his own experience and observations. In summing up his reflections, however, on the pathology and etiology of the form of pneumonia under review, he indicates, in a note, several authorities, to which, we suppose, he intended to refer the reader on this head, and which we shall now proceed to examine.

1. *Broussais.*—Dr. Manson states that this author

"Doubtless saw a vast number of cases of this disease whilst serving in the French armies, and has noticed at great length the connection between malarial fever and pneumonia."

Broussais says that when he took charge of one of the hospitals of Bruges, he found a great number of patients, in whom *intermittent fever* had ceased, and who were waiting the restoration of their strength. "Some of these patients," he continues, "having died, I hastened to examine their bodies, from which so much was to be learned. What was my astonishment, after five or six autopsies, at *constantly finding the lungs* HEPATIZED! In some, the violence of the cough had prepared me for this appearance; but, on the other hand, there were several in whom I observed the effects of the disease, without having had a previous suspicion of its existence. I then observed, with the utmost attention, those patients whose actual state might lead me to the fear of a similar result. * * * * I was soon convinced of the analogy between these patients and those in whom I had found *hepatizations* after death. * * All eventually died, and dissection showed that the principal lesions were, *indurated lungs, and an inflamed pleura.*" In speaking of these cases further on, he again states—"1st, That they had experienced several relapses of intermittent fevers, which had always been combatted by large doses of the great febrifuge; 2d, That they had been free from fever for a certain time, as fifteen days, a month, &c.; 3d, That they had a dry, nocturnal cough, with a straw-colored complexion, and a slight tumidity; 4th, That one-half perished suddenly, without fever or any other violent exacerbation; 5th, That a *post-mortem* examination *always*

showed me *induration of the parenchyma*, and sometimes *inflammation and exudation of the pleura.*" (*a*)

It seems to us that Broussais' experience in regard to the "*anatomical character*" of the pneumonia that he saw, in connection with malarial fever, differs very widely from the speculations of Dr. Manson. *Broussais* found the lungs "*constantly hepatized.*" *Dr. Manson* denies that they are *hepatized* at all! But *Broussais* made *autopsies;* and it does not appear that *Doctor Manson* has. The former speaks from positive knowledge; the latter speaks from fertile conjecture.

2. *Cleghorn.*—It will be remembered that, in his section on "*Diagnosis,*" Dr. Manson could "not refrain from adding a striking characteristic as drawn from Cleghorn," in respect to the sudden supervention of a furious *delirium* "on the fourth or fifth day," during which, unless relieved by nature or art, "the patient expired, either suffocated or raving mad"; and that, in commenting on Dr. Manson's recognition of this remarkable feature of Cleghorn's cases in the "malarial pneumonias" of our lay, we stated that, according to Dr. Manson's own account, the *pathological condition of the lungs* in those of the former differed very widely from that in those of the latter. (See April number of this Journal, pp. 410, 421). We have now an opportunity of verifying this statement, and, at the same time, of pointing out, in another aspect, how unfortunate is Dr. Manson's reference to the author in question. Cleghorn states—

"I examined fourteen of the bodies (out of twenty-one patients whom I lost by this distemper), in all which the lungs were principally affected, whilst in several the *pleura* was perfectly sound, or only slightly attached to the lungs, which is a common case in adults, as every one knows, who is accustomed to dissections. In many, the lungs were converted into a hard *liver-like* substance, and *sunk in water;* in some the diaphragm was inflamed; in others, large firm *polypi* were taken out of the ventricles of the heart and large vessels adjoining. Abscesses, with sanious *ichor*, and a rotten gelatinous substance, instead of concocted matter, were frequently found, *even in those who died so early as the fourth day*, either in the lungs, or between the lungs and *pleura*, where they adhered, or between the membranes of the *mediastinum*, near the diaphragm. And these abscesses had sometimes emptied themselves into the cavity of the thorax, so that the

(*a*) History of Chronic Phlegmasiæ, by F. J.V. Broussais, vol. i. 98–106.

lungs floated in purulent serum; their external membrane, and likewise the pleura, being greatly thickened, and converted, as it were, into a white crust, like melted tallow grown cold, part of them being grown cold, part of them being eroded and detached from the rest. In some, the pericardium was full of purulent matter, its internal membrane and the outer surface of the heart being affected in the same manner as I have just now described those of the *lungs* and *pleura*. In two people whose heads were examined, the sinuses of the *dura mater* were stuffed and distended with blood, the membrane itself being sound, and the *pia mater*, together with the *plexus choroides*, was inflamed, and much thicker than in a natural state." (*a*)

We doubt if there be upon record an account of the ravages of a more violent pneumonia, than that just presented from the work of Cleghorn. In what respect it confirms the pathological views of Dr. Manson, we are utterly at a loss to perceive. Considering the time at which Cleghorn wrote, it is remarkable for the graphic power of its delineation. In many cases, says he, the lungs were converted into a hard *liver-like* substance, and *sunk in water*,—the *hepatization* of modern authorities; in some the diaphragm was inflamed; and in others, large firm *polypi* were found in the heart,—constituting the yellow, elastic, dense, closely adherent concretions, intertwined among the columnæ carneæ, which Grisolle met with in one-fifth of the subjects dying of pneumonia examined by him, and which, from the great frequency of their occurrence, Bouillaud sought to deduce the *law*, that they constantly exist in the second stage of the disease. In some, abscesses were found even in those who succumbed so early as the fourth day; and the *pleura* was greatly thickened and covered with a yellowish white crust, like melted tallow grown cold—the false membrane of our times,—and the lungs floated in an effusion which Cleghorn regarded as purulent. In this complication with pleurisy, we recognize a well-known occasional accompaniment of the pneumonia of our day; since of 247 cases of pneumonia seen by Grisolle, *thirty-one* were complicated by pleurisy,—the application of this word being restricted to cases in which the pleuritic effusion is sufficiently extensive to produce distinct physical signs. In others of Cleghorn's cases, we perceive the effects of *peri* and *endo-carditis;* another point of resemblance to ordinary pneumonia, as of fifty-eight of Gri-

(*a*) Diseases of Minorca, 267-269.

solle's patients, *twelve* presented various morbid phenomena referable to the heart and great vessels; and in three of these twelve cases the affection was *pericarditis* (fatal in two instances); and in eight (exclusive of those seized with pericarditis) a modification in the quality of the sounds of the heart was detected.

Now, let the reader contrast Cleghorn's account of the *pathological anatomy* of his "malarial pneumonia," with Dr. Manson's statement respecting that described by himself, in which Dr. Manson says (in speaking of the *first stage*), "the *anatomical character* of this affection is doubtless an *intense congestion* of the parenchyma, associated, *perhaps,* in *some degree,* with inflammation"; and (in speaking of the *second stage*), that "the state of the lung parenchyma partakes more of the character of *congestion* than *active inflammation*"! Is it possible that Dr. Manson could have made a more unfortunate reference than to this author?

3. *Lænnec.*—Dr. Manson states—

"In Lænnec's cases, he only mentions "that *traces* of inflammation" were found after death" (Diseases of the Chest, 249).

While discussing Dr. Manson's description of the Physical Signs of the "stage of *engorgement,*" we had occasion to quote the passage from Lænnec's works, to which reference is here made. In speaking of the "*pernicious fevers* denominated pneumonic," Lænnec states that the morbid anatomy of the affection is yet very imperfectly known, from the circumstance that this fever is rarely fatal; as we fortunately possess in cinchona, when administered in time, a certain means of cure; but that "we have some facts, however, which prove that traces of *pneumonia* have been found in subjects dead of this disease"; and without furnishing any other evidence of the allegation, he adds—"I have myself, in two accessions of *this fever*, witnessed the presence of the glutinous sputa, and a very intense crepitous ronchus." Now, what is this but to state that in those *pernicious fevers*, which, from the lungs offering the predominant symptom, have been termed pneumonic, "traces of *pneumonia*" have been found after death? If from cases of this character we are to deduce the pathology of the pneumonia of the South, then, instead of its being an idiopathic disease, it is merely an inter-

current affection supervening in, and complicating, the career of that class of periodic fevers which have been termed *pernicious!* The mere statement of this proposition is sufficient to refute it. It follows, therefore, *ex necessitate rei*, that any deduction made from the observation of the class of cases in question, and applied to the elucidation of the morbid anatomy of a very different class, is, simply, a complete *non-sequiter*. We shall presently see, however, that when pneumonia supervenes during the course of *remittent fever*,—the phenomena of which, when blended with "the signs and symptoms of the thoracic lesion," constitute the "malarial pneumonia" of Dr. Manson,—something more than mere "*traces* of inflammation" are found at the examinations *post-mortem*.

4. M. Bailly's *Traité Anatomico-Pathologique des Fièvres Intermittentes*, &c., is the only other work to which reference is made by Dr. Manson in this connection; but as we have not been able, as yet, to obtain it, we can neither verify nor disprove the appositeness of the reference. But if it be like the others referred to by Dr. Manson, under this head, the reader may well judge what support it affords to his views.

We have now examined, with the exception aforesaid, all the authorities, in this relation, to which Dr. Manson has indicated reference. We deem it entirely unnecessary to enter into any extended comments on them. The reader has already seen how far they fall short of sustaining Doctor Manson in his views of "*the pathological condition of the lung*" in "malarial pneumonia." It seems to us that they are, of themselves, sufficiently decisive of the question at issue; but as there are others readily at hand, it may be as well, although, perhaps, a work of supererogation, briefly to adduce them.

In regard to the *morbid anatomy* of *primary* and *febrile* pneumonia,—using the latter term to designate the pneumonia which complicates intermittent and remittent fever,—Dr. Morehead states—

"There was a *post mortem* examination made in 15 of the 24 fatal cases of primary pneumonia, and in 7 of the 8 fatal cases of febrile pneumonia. In 11 of the cases the solidified lung was found in a state of induration,

either red or gray: this condition, compared with readily lacerable hepatization, occurs in this hospital in a greater proportion even than this series shows. It will be found to be related to asthenic states of the system, to the disease running a slower course, and not unfrequently to advanced period of life. True hepatization, on the other hand, will be found to have occurred in better states of the constitution, and in instances in which the fatal issue has taken place sooner: in the febrile form it exists in a greater proportion than induration. Of the 7 cases of febrile pneumonia there was hepatization in 5, and induration in 2; whereas in the 15 cases of primary pneumonia there was induration in 9, and hepatization in 6. The question of whether the gray induration is to be regarded as an advanced stage of the red or brown, or a distinct variety from the commencement, has been discussed at different times. These cases, I think, rather countenance the former view of the pathology of this change." (*a*)

Such is the testimony of this accomplished physician and pathologist, derived from actual inspection of the lungs at *post mortem* examinations; and how different is it from the hypothetical statements of Dr. Manson? Dr. Manson denies that the term *hepatization* can be properly used to designate "condensation of the lung in this disease" ("malarial pneumonia"); and yet *true hepatization* existed in a greater proportion of this class of cases in Prof. Morehead's experience, than in ordinary primary pneumonia. But Prof. Morehead looked at the lungs after death, and counted his cases; while Dr. Manson, on the other hand, bases his affirmations upon the vague hypothesis of mere clinical deductions.

It is the *remittent* form of periodic fever, be it remembered, which, being associated with pneumonia, constitutes the "*malarial* pneumonia," *par excellence*, of *Dr. Manson*. Let us see, then, what are the results of *post mortem* examinations in this particular class of cases. We remark, in the first place, however, that pneumonia does not appear to supervene very often in the course of remittent fever. Bartlett cites Bailly as mentioning the lungs in only a part of his cases, and in nearly every instance where he does so, calls them healthy; and Maillot found the lungs as free from disease as after death from any acute affection (*b*). Dr. Stewardson,—whose name is promi-

(*a*) Clinical Researches on Disease in India, by Charles Morehead, M. D., vol. ii. 322, '3.

(*b*) Fevers of the United States, by Elisha Bartlett, M.D., p. 322.

nently, if not inseparably, connected with the morbid anatomy of *remittent fever* in this country,—makes a similar statement. The condition of the lungs, in this fever, says he, is much the same as in most other acute diseases, not especially seated in these organs. Indeed, we may adduce the authority of Doctor Manson himself, to the same purport; for, in his essay on *Remittent Fever*, he makes no allusion whatever to a *pneumonic* complication. And, be it remembered, this essay was penned for the express purpose of furnishing a correct description of this fever, "as it now presents itself," since, according to Doctor Manson, none can "be found in any standard work." It is true that, in the "INITIAL chill" of the ordinary form of remittent fever, he states that the "breathing is accelerated, in some cases unequal, and interrupted by sighs"; and in the grave form (*congestive fever*, *pernicious fever*, *algid fever*), when, as he conceives, the *cerebellum*, particularly, is invaded, that, in the cold stage, "the respiration is irregular and laborious, the expirations being forcible and prolonged"; but, he adds (in a note), although "these symptoms have been attributed to *pulmonary engorgements*, it is now *known* that the lungs, after death, *very rarely present the slightest indication of such a condition having existed*,—the dyspnœa is, therefore, evidently owing to *impeded innervation*, and this proceeding from a *congested condition of the vessels of the cerebellum*. (*a*) When, however, pneumonia is developed during the career of remittent fever, the concurrent experience of independent and excellent observers fails to sustain the pathological views of Dr. Manson respecting its true nature. Thus, of the five fatal cases of febrile pneumonia reported in detail by Morehead, while there was general bronchitis in one, and the pneumonia did not pass beyond the first stage; and, in another, presented the posterior part of the right lung in a state of red *hepatization*, in parts arranged in a lobular form; yet, in the remaining three, it passed into the second stage of red *hepatization*, and even into the third stage in two of them. (*b*) In six out of seven fatal cases of remittent

(*a*) Transactions of the Seventh Annual Meeting of the Medical Society of North Carolina, pp. 46, 47, 54. (*b*) Op. cit., vol. ii. 342–347.

fever, in which the lungs are particularly described, Stewardson found *hepatization* in one case (*a*); and subsequently to the publication of his papers on remittent fever, he saw one case of this fever at the Pennsylvania Hospital, in the Fall of 1842, which proved fatal only by the supervention of pneumonia of the lower lobe of the left lung, which was found *hepatized* throughout nearly its whole extent. (*b*) Of five fatal cases of remittent fever observed by Swett, in the New York Hospital, pneumonia was the immediate cause of death in two. In one, the lower lobe of both lungs was in the first stage of pneumonia, passing into the *second stage* in some points; and in the other, both lungs were the seat of pneumonia, the lower lobes being principally affected, although in the right lung the inflammation had climbed up the posterior portion to the very apex, and was of the lobular form, presenting nodules of gradulated and softened texture of a light red color, surrounded by portions of a brownish-red or livid color, in some parts dry, in others giving out a viscid reddish fluid more or less aërated. (*c*) Of twelve cases of remittent fever that terminated fatally in the Baltimore Alms House Hospital in the summer and autumn of 1844,—reported by Drs. Anderson and Frick,—pneumonia occurred in one, and was the cause of death. A man, ætat. 45, entered the hospital September 21st, from an extremely malarious district, and presenting all the symptoms of remittent fever. He stated that he had chills and fevers two or three weeks previously, but that, two days ago, he had again another chill, succeeded by headache, fever, and vomiting. On the 26th, auscultation detected a friction sound in the lower part of the right lung behind, with *subcrepitant râle* in the middle and upper part posteriorly; dullness over the whole lung, more marked in front; bronchial, almost tubal blowing in some parts of the lung; expectoration of a prune-juice color, &c. Death ensued on the 28th—the 10th day of his illness. Now for the *autopsy*. "The whole of right lung in the *third stage* of pneumonia; pulmonic abscesses

(*a*) American Jour. Med. Science, N S., vol. i. 300, 301.
(*b*) Note to Elliotson's Practice, 345.
(*c*) American Jour. Med. Science, N. S., vol. ix. 35, 37.

occupy upper and middle lobes; bands of vessels and cellular tissue traversing the cavity; no tubercles were discovered." (*c*)

In consideration, then, of the facts, arguments, and authorities now brought forward, it seems to us as if one cannot resist the conclusion that the matter under discussion can scarcely be deemed, with any propriety, an open question. It appears to have been demonstrated, beyond the power of successful controversy, that, when pneumonia occurs in connection with malarial fever, the lungs do become *hepatized*, and that the solidity *is* chiefly due to an "*inflammatory process.*" We are not disposed to deny that solidification, in certain cases of pneumonia, whether they be impressed by a malarial or periodic fever influence or not, may be, in part, produced by intensé congestion of blood in the capillaries of the lung; but there is, in addition, inflammatory exudation into the air-cells, and to a slight extent, also, into their walls; and any view of the nature of this solidity, which does not give prominency to the products of inflammation, is, in our judgment, utterly at variance with the teachings of sound pathology.

We pass on to the

"DIAGNOSIS BETWEEN MALARIAL AND OTHER FORMS OF PNEUMONIA."

Under this head, Doctor Manson proceeds to give the differential diagnosis between "malarial," simple sthenic, and typhoid pneumonia. Between the two former, as we have already premised, we have noticed the distinctive features, as we compared the appearance and mode of succession of the vital symptoms. We have now to examine the diagnostic differences between "malarial" and *typhoid* pneumonia. Dr. Manson states that—

"By the term typhoid pneumonia, I designate that species of pulmonic lesion which presents itself as a complication of the form of continued fever for many years widely prevalent in the Old and New World, and by general acceptation known as typhoid fever. It is true that this term and its synonymes have often been applied to simple and malarial pneumonia, but I believe on erroneous grounds. The designations typhoid, asthenic or sthenic, cannot be applied to the latter disease; as the exhibitions of vital force or reaction are too variable in their nature, intensity and duration, to render

(*c*) American Jour. Med. Science, N. S., vol. xi. 319, 320.

them appropriate. Thus, as we have seen in some cases of the malarial disease, the vital powers were almost overwhelmed at its onset, continuing thus for days together, and at last, perhaps, followed by vehement reaction; whilst in the cases generally there were such diurnal changes that the same patient might have been regarded in an asthenic or sthenic on the same day, according as he may have been seen in the remission, chill or exacerbation. It is true that the malarial affection, when continuing, finally places the patient in an adynamic state, but this is true of acute diseases generally, and must be regarded as a sequent and not as an original condition.

The term typhoid cannot be properly applied to any form of simple inflammation. In my opinion, pure inflammation of the lungs, occurring in a previously healthy subject, cannot present typhoid symptoms, unless it is associated with some morbid element which impairs the resistant powers of the organism. When, therefore, such a patient prominently presents the symptoms of pneumonia, attended with deficient vital reaction, the inflammation of the lungs is a secondary or collateral affection, and not the primary or essential malady under which he is laboring.

Typhoid pneumonia may be presented as an intercurrent affection, but often the inflammation of the lungs is the first and most conspicuous symptom. To such an extent and so marked is the expression of this lesion, that the practitioner is liable to regard the pulmonary affection as the sole cause of the illness of the patient, and not perceiving the system condition with which it is connected, may be led into the error of treating it as such. On minutely inspecting the case, however, he will be enabled generally to discern its true character, and recognize under the veil of pneumonia the peculiar form of fever on which it is engrafted. To the novice we offer a few observations as guides to a correct diagnosis. Typhoid pneumonia usually differs from the forms before mentioned—1st, In its mode of access. It is usually more gradual in its approach, and in cases where the pneumonic symptoms announce themselves suddenly, it will be commonly found, on inquiry, that the patient has been indisposed for some time before their appearance; 2dly, In the character of the attendant fever. The regularly recurring paroxysms of malarial pneumonia are absent; the firm, full pulse of the simple form is never observed; but, on the other hand, symptoms of great depression of the vital powers are generally present. In short, we are usually enabled to discover the peculiarity of the case, by bearing in mind that the patient before him is the subject of typhoid fever with pneumonia superadded."

This division of the subject need not detain us long; but (as we conceive) there are some errors emitted by Dr. Manson, in the above extract, which should not be passed unnoticed.

1. It has doubtless occurred to the reader that, while in his definition of *typhoid pneumonia*, Dr. Manson includes only that form of pneumonia which complicates the career of *typhoid fever*, he admits, nevertheless, further on, that "pure inflammation of

the lungs, occurring in a previously healthy subject," may present "*typhoid symptoms*," when it "is associated with *some morbid element* which impairs the resistant powers of the organism." How, then, could he affirm, in the sentence immediately preceding that containing the latter statement, that "the term *typhoid* cannot be *properly* applied to *any form* of simple inflammation"? Indeed, he had informed us, on the second page of his Essay, that "inflammation of the lungs, like inflammation of any other structure, may be altogether different in its *nature*, complications and terminations." According to Dr. Manson, himself, therefore, it is an obvious error, and a complete *non sequiter*, to assert — as he does in the above quotation,— that when pneumonia is accompanied by the signs of "*deficient vital reaction*," the inflammation is, of necessity, "a secondary or collateral affection, and not the primary or essential malady." And why should not an inflammation of the lungs be attended by the signs of an asthenic condition, as well as an inflammation of any other structure? Is there no other element of a depressing nature, save the particular element of typhoid fever? Every one knows that, in ordinary medical *parlance*, the terms asthenic and typhoid are synonymes; and they are so used by Dr. Manson. We constantly hear of diseases assuming a *typhoid type*. Thus, we have an asthenic or typhoid dysentery: why not, then, a *typhoid pneumonia*, irrespective of the special influence of *typhoid fever?* It is well known, in fact, that, in certain states of the system, pneumonia is almost always attended by the signs of adynamia. Has Dr. Manson never met with the asthenic pneumonia of drunkards? It is very true, that there cannot be an asthenic or typhoid pneumonia, independent of the presence of a depressing element; but that element may be, and often is, primary in its action, and wholly unconnected with the special diathesis of typhoid fever. This truth, if we mistake not, is generally admitted. (*a*) Dr. Wood states that he treated

(*a*) Drake, Prin. Dis. of Inter. Valley of N.A., 867; Barlow, Man. Prac. Med., 203; C. J. B. Williams, Cyc. Prac. Med., vol. iii. 611; Dunglison, Prac. Med., vol. i., first ed., 317; Stokes, Dis. of Chest, 303; Gerhard, Dis. of Chest, second ed., 154; Blakiston, Dis. of Chest, 301–'3; Wood, Prac. Med., vol. ii. 18; Condie, Note to Watson's Prac., third ed., 602; Mackintosh, Principles of Pathology

between thirty and forty cases of *typhoid pneumonia* in one season, in an institution in Philadelphia, the cause of which, so far as it could be ascertained, was an excessively offensive exhalation which filled the whole house, and proceeded from a cellar into which the contents of a privy had been discharged. (*b*) Typhoid pneumonia, indeed, often prevails as an epidemic, striking down persons of every age, without warning, and in the midst of the most perfect health. It is secondary to no disease whatever, but obviously the result of some latent endemic, or wide-spreading epidemic influence, of the nature of which we are often confessedly ignorant. An epidemic of this kind is described as having appeared twenty-five miles south of Raleigh, N. C., in January, 1853, and extending through a pine and sandy country for twenty miles. The invasion was often sudden, sparing neither old nor young. Many laborers were attacked while at work, after having ate a full meal; and in others, the disease commenced at night, after retiring to rest. (*c*)

Nor, on the other hand, is the pneumonia "which presents itself as a complication of the form of continued fever for many years widely prevalent in the Old and New World, and by general acceptation known as *typhoid fever*," always of an *asthenic* type. During our service in the Hospital of Baltimore City and County Alms House, in 1843, as one of the resident physicians, William Davis, ætat. 17, of vigorous constitution, and never before, as he stated, the subject of an attack of sickness, entered the wards in the month of June, ill of typhoid fever of a highly sthenic grade, during the course of which double pneumonia supervened. The treatment was directed by the late Doctor Wm. Power, Professor of the Theory and Practice of Medicine in the University of Maryland,—a rigid and conscientious observer of the school of Louis and Chomel; and the patient was seen several times, also, by Dr. Thomas H. Buckler, of Baltimore,—well known as one of the most acute and accomplished

and Practice of Medicine, fourth Amer. edit., 392;] James R. Bennett, Amer. Jour. Med. Sciences, N. S., vol. xxv. 472; Gibbes, Amer. Jour. Med. Sciences, N. S., vol. iv, 289, etc.

(*b*) Op. cit., vol. ii. 21.

(*c*) W. H. McKee, Virginia Medical and Surgical Journal, vol. iii. 97.

practitioners in this country. The pneumonia was combated by three bleedings, and other active antiphlogistic agents, and, finally, yielded; but some days afterwards, the patient succumbed to the typhoid fever. There could be no mistake in regard to the diagnosis; for the *autopsy* revealed the most conclusive evidences of the character of the case,—extensive ulcerations of the glands of Peyer, &c.

We conclude, therefore—Dr. Manson to the contrary notwithstanding,—that there *is* a form of typhoid (asthenic) pneumonia, which occurs unconnected with, and entirely independent of, the particular diathesis of *typhoid fever;* and that the pneumonia which sometimes supervenes in this fever is not, *ex necessitate rei*, of an asthenic type.

2. Dr. Manson states further that—

"The designations typhoid, asthenic or sthenic, cannot be applied to malarial pneumonia, as the exhibitions of vital force or reaction are too variable in their nature, intensity and duration to render them appropriate. Thus, as we have seen in some cases of the malarial disease, the vital powers were almost overwhelmed at its onset, continuing thus for days together, and at last, perhaps, followed by vehement reaction; whilst in the cases generally there were such diurnal changes that the same patient might have been regarded in an asthenic or sthenic [condition ?] on the same day, according as he may have been seen in the remission, chill or exacerbation."

In his paper on remittent fever, to which we have already repeatedly referred, Dr. Manson speaks of an "*adynamic*" form. (*d*) Why not admit an adynamic or asthenic "*malarial pneumonia*," as well as an adynamic or asthenic *remittent fever?* In both there are daily remissions and exacerbations of the fever, and the patient offers more heat of skin, and a fuller and firmer pulse, in the exacerbations of either. If, in "adynamic" *remittent fever*, as Doctor Manson states, the "exacerbations and remissions are indistinctly marked", so, also, does he state that, in "*malarial pneumonia*," the "remissions are sometimes obscure, and an active fever exists during the whole day and night." In both diseases alike,—in remittent fever, and in "malarial pneumonia",—the grade of the febrile movement is represented to be the same, by the same observer,—the type-

(*a*) Transactions of the Seventh Annual Meeting of the Medical Society of the State of North Carolina, 1856, p. 51.

character of the pulse being described as specially "*compressible.*" And if, during the *access*, in "malarial pneumonia," the skin is hotter and the pulse fuller and stronger, than during the *remission*, still, it is, according to Dr. Manson, "*soft, of moderate fullness, compressible, and never or rarely very full, tense or hard.*" Indeed, Dr. Manson's description of "malarial pneumonia," all along, presents the characteristics rather of an asthenic than sthenic disease; and such, also, is the character of the pneumonia which has prevailed in this section, as well as other regions of the globe, for some years past, whether it has presented the tokens of a malarial or periodic fever element, or not. Is not this the experience of almost every one?

In regard to those cases of "malarial pneumonia," in which "the vital powers are almost overwhelmed at the onset, continuing thus for days together, and at last, perhaps, followed by *vehement reaction*," it is evident that, according to Dr. Manson himself, they are of rare and exceptional occurrence; for, in his remarks on the diagnosis between simple and "malarial pneumonia," he contrasts the full, strong and hard pulse of the former, with the soft, compressible, and never or rarely very full, tense or hard pulse of the latter. And if, as he asserts, the latter differs "*in character and nature*" from the former, it is for him to account for its presenting, "in some cases," the signs of "*vehement reaction.*" His objections to "the designations, typhoid or asthenic" being applied to "malarial pneumonia," are in sad contrast with his description of the general character of the disease; and if the cases that offer evidences of "vehement reaction" present any bar to the proper application of the "designations" in question, so, also, do those cases conflict with the fundamental character of the disease under notice, as described by himself.

Such, only, are some of the dilemmas, under this head, into which Dr. Manson is driven, by his effort to erect his "malarial pneumonia" into a disease radically distinct and different from all other forms of pneumonia. He cannot attempt even to define it, and to assign reasons why certain "designations," in ordinary use, should not be applied to it, without rendering those

dilemmas as obvious as they are embarrassing. Let him admit that any form of pneumonia may occur in connection with periodic or malarial fever, and that the particular form or type of pneumonia which occurs, in different cases, is determined by general or collateral circumstances, and all these difficulties, as well as many others, vanish at once.

3. Dr. Manson continues—

"Typhoid pneumonia may be presented as an intercurrent affection, but often the inflammation of the lungs is the first and most conspicuous symptom. To such an extent and so marked is the expression of this lesion, that the practitioner is liable to regard the pulmonary affection as the sole cause of the illness of the patient, and not perceiving the system condition with which it is connected, may be led into the error of treating it as such. On minutely inspecting the case, however, he will be enabled generally to discern its true character, and recognize under the veil of pneumonia the peculiar form of fever on which it is engrafted. To the novice we offer a few observations as guides to a correct diagnosis."

That pneumonia *may* present itself soon after the onset in typhoid fever, may be very true; but, as a rule, it supervenes at a much later period. It is stated by Flint—who, according to high Southern authority (*a*), is "*the Louis of America*"—that "the cases must be *extremely rare* in which the lungs become inflamed at the commencement, or during the earlier part of the febrile career." (*b*) And Drake affirms that *all observers* concur in representing that pulmonary lesions do not in general exist from the beginning of continued fevers, but insiduously supervene during their progress, being, in fact, secondary affections. (*c*) Indeed, Dr. Manson, himself, virtually admits very nearly the same thing when he declares that *typhoid pneumonia* usually differs from simple and "malarial pneumonia" in its mode of access, which is generally "more *gradual* in its approach, and in cases where the pneumonic symptoms announce themselves suddenly, it will be commonly found, on inquiry, that the patient had been indisposed *for some time before their appearance.*" It must be very seldom, then, and *not* "often," as Dr. Manson first, and positively, avers, that the practitioner will encounter pneumonia as "*the first and most conspicuous*

(*a*) Editor of New Orleans Med. and Surg. Jour., vol. ix. 680.
(*b*) Clinical Reports on Continued Fever, 300. (*c*) Op. cit., 489.

symptom" in typhoid fever; and if the "*novice*"—in whose behalf Dr. Manson seems especially to address himself on this branch of his subject—should heed Dr. Manson's injunction, and seek to "recognize, under the veil of pneumonia," the phenomena of *typhoid fever*, in the many instances of asthenic pneumonia, he is so likely to meet with in these latter times, he will not only have no cause to be thankful to Dr. Manson for his "guide to a correct diagnosis," but will "often" find that, in following him in this regard, he has fallen into a practical error just the reverse of that pointed out by his "guide"; and has mistaken a primary substantive disease, for an intercurrent affection, supervening in a malady which has no connection whatever with the case before him. Dr. Manson may have observed some instances of pneumonia, engendered by its ordinary exciting causes, just as the patients were on the eve of an attack of typhoid fever; but the occurrence of such rare and purely exceptional cases cannot set aside the well-established pathological law, that the pneumonia which sometimes occurs in typhoid fever, supervenes, as the rule, at an advanced period, if not late in the career of that disease.

(To be continued.)

Luxation of Eye.—M. Reyssie relates the following case: During a conflagration the patient received the full stream from a fire-pump in his face. The column of water struck the eyelids of the right eye with violence, thrusting them strongly backwards. Contracting under the double influence of the shock and the cold, they forcibly compressed the globe, forcing it out of its orbit by a kind of enucleation. The author saw the patient in an hour, and found the eye hanging out, retained only by its muscles and distended optic nerve. Its reduction was very easy. Local antiphlogistics and aperients were employed, and in the course of ten days he saw as well as before the accident. Seen thirty months after the accident, he continued quite well.—*Gaz. des Hôp.*, No. 65.

TO THE EDITOR OF THE MEDICAL JOURNAL OF NORTH CAROLINA.

PITTSBORO, N. C.

Dear Sir:

I send you notes of a case which, if you deem worthy of a place in the Journal, I would be pleased to see reported.

March 15th, '59—Was called to J. F., a "Quadroon," aged 22. Taken with premonitory symptoms of labor eight hours previous to my visit; pains trivial and purgative; and, with exception of an obliquity of the uterus and an undilating os, her general condition favorable. Being her first accouchement, she was allowed to proceed, after the obliquity was corrected, until the 16th, when ext. belladonna was applied to the os uteri. The pains, which, up to this time, had been very feeble, ceased; as soon as the belladonna produced dilatation, ergot was administered, which acted finely, bringing the *head* well down, and, with assistance, she gave birth to a monster. Previous to its expulsion, my attention was arrested by a peculiarity in the shape of the occiput; a sensation was communicated by the touch, as if the cord greatly corrugated, had been folded across the occiput, and considerable force had to be exerted to extract the child. As soon as it was exposed, these difficulties were explained.

It presented the following appearances: From a little below the point of the occiput, a bloody tumor protruded, consisting of distinct sacks, containing only a small portion of serum, and communicating directly with the brain. The sacks reached nearly to the shoulder. It had a double hair-lip, which destroyed the roof of the mouth, palate, &c. Its abdomen was immense, the walls of which were protruded by two large tumors, occupying the right and left lumbar regions. Organs of generation of neither sex; a rudimentary penis, with a cleft in it; a scrotum; its limbs, eyes and ears perfect. Upon examination, the abdominal tumors proved to be the kidneys, filled with *hydatids*. *Each* kidney weighed 15 ounces; length, 6½ inches; width, 4½ inches, and 2¾ thick. Heart and lungs healthy; liver and intestines (save adhesion) normal; no bladder could be detected. This monster was nearly dry; not a wine-glassful of

the water ever escaped from first to last, yet it would have weighed eight or nine pounds, and lived and cried for a half hour. Though entitled by nature (and *diseased supra renal capsules*) to be dark, it was a very white child, with blue eyes.

Very respectfully, &c., &c.,

D. S. WADDELL.

PS.—Both kidneys alike.—D. S. W.

CONDITION OF KIDNEYS.

Each kidney was enveloped in large loose sacks, which were filled with a substance resembling fish spawn. Beneath the sack hydatids of different sizes and shapes filled the whole substance of the kidney. The *external* coat of the largest of these hydatids was covered with granules. Whether they were echinococci or no, I was unable to determine, being without a microscope; though echinococci are generally found *within* acephalocysts. Within the larger hydatids acephalocysts were plainly observed. The contents of some of them perfectly limpid, in others, opaque. These tumors were of a yellowish color, shaded off to a pearl color, thereby showing that the theory of *age giving* these colors to hydatid tumors, to say the least, is a doubtful one. The remains of obliterated blood vessels were noticed, yet the remaining vessels were numerous, and greatly enlarged in calibre. These tumors, when punctured, would spirt out their contents.

The age at which this disease was developed (if we can term inter-uterine existence of any age), is truly remarkable, as there is no case on record where it has occurred under five or six years. Its *rare* appearance in the kidneys (for it is *very seldom* found in that locality), together with the fact that the kidneys *alone* were infected with this disease, renders the case an interesting one.

Bi-carb. of Ammonia.—Mr. Witt, of London, has published a pamphlet to prove that bi-carbonate of ammonia is a never-failing remedy in scarlet-fever and measles.

A REPORT OF THE DISEASES OF PERQUIMANS COUNTY.

BY DEVIT COX, M. D.

Since the last meeting of our Society, we have not had our usual amount of practice; and for the last two years it has not been chiefly confined to the Fall month, as formerly, but has been almost equally distributed over the several months of the year.

The falling off in the extent of our autumnal practice, accounts for the want of our usual number of cases.

The type of our diseases has been adynamic, and required, almost invariably, stimulating and tonic treatment. I don't remember of having used the lancet over a half dozen times during the past year.

Dysentery and Diarrhœa were almost entirely confined to May, June and July, During these three months they prevailed quite extensively, but were of a mild form, and generally yielded to medicinal agents readily. The liver and portal circulation were involved, and perhaps the principal cause of the disease. When the patient could be seen in the first stage of the disease, ten grains of calomel were given, followed, in six or eight hours, with a dose of castor oil. This, very often, was all the patient required; but if the disease went on, the patient continued to discharge bloody mucus, and other unpleasant symptoms were present: he was put on calomel, ipecac. and opium, until the symptoms ameliorated, or his gums were slightly affected. Once in two or three days his bowels were swept out with a dose of castor oil, and twenty drops of the oil of turpentine, to carry off the irritating matters. If the tongue became dry, and had the appearance of cracking, the patient was put immediately on the oil of turpentine—six drops every four hours, in mucilage of gum arabic.

Acetate of lead was also sometimes found useful, especially in diarrhœa. Blisters to the abdomen, followed with emollient poultices, were found useful; and if the patient was troubled very much with tenesmus, injections of flax-seed mucilage, with laudanum, were used with advantage.

We have had Continued Fever and Erysipelas during the past year, but they have not prevailed so extensively as formerly, and in appearance were not unlike the epidemics of the year 1857, and called for a similar treatment. A very interesting report was made before this Society, by my friend Dr. Caleb Winslow, giving a full description of them, and the most approved treatment.

Bilious Remittent and Intermittent Fevers have prevailed in a mild form throughout the whole of the past year: no season was exempt: not even the winter. They yielded readily to the usual treatment.

Pernicious Fever, known, perhaps, better by our physicians as the Congestive Chill, has prevailed, for the last two or three years, to a much greater extent than usual. Fall before last, at one period of time, it appeared to take the place of the remittent and intermittent fevers almost entirely. The patient may be attacked without any premonitory symptoms; but generally there are some symptoms of the disease which manifest themselves several days prior to the occurrence of the first paroxysm, such as slight headache and a general depression of the feelings, with some pain or aching sensation of the back, at the same hour of the day the first paroxysm makes its appearance.

I have no doubt but that the mild intermittent and remittent fever are capable of being converted into the pernicious, as it is not uncommon for a patient to have several mild paroxysms before an alarming one makes its appearance.

The disease comes on with a chilly sensation, an indescribable, wretched feeling, with nausea, followed with vomiting of the contents of the stomach, and everything that is taken on it. The patient has an unquenchable thirst for cold water, or cold drinks of any kind, and, as soon as taken, rejects them. Very soon he complains of heat—heat all over,—but more particularly of internal heat; his stomach and abdomen suffer very much, and he begs for cold water to cool them. In the most alarming cases the bowels are very active, having to evacuate them for every twenty or thirty minutes.

The first evacuations are of a thin, watery character, contain-

ing some little rolls of the mucous membrane; but as the evacuations of the bowels go on, the discharge changes to a bloody serum, and sometimes almost clear blood is evacuated. The patient has an alarming expression about his countenance,—his eyes and face have an unnatural look. Though he complains of being hot, yet his hands, his feet, and indeed all of the extremities, are cold and clammy. Bathed in a cold sweat, sometimes it stands in large drops all over the face and breast.

Circulation is very feeble; frequently there cannot be any pulsation detected at the wrist; and, indeed, the circulation of the extremities has quite stopped. The tongue is coated with a white or yellowish white fur, generally dry and sometimes cold to the touch of the finger.

The great object in the treatment of this disease, as the practitioner is most always called during the paroxysm, is to bring about reaction, and then to use remedial agents to prevent a return. External as well as internal use of medicine is found indispensable in this disease. After placing the patient in a hot mustard bath, to his knees, put over the whole of his abdomen and chest a mustard plaster, and place a person to each of his upper extremities, with oil of turpentine and African pepper, to rub them as long as he can bear it. Place then a mustard plaster the whole length of the spine. Give him two grains of calomel, down two or three grains of acetate of lead, and one grain of opium. Now give him brandy—old apple, if you can get it,—and as much as he can bear, occasionally giving with it laudanum. After a little while, take the patient's feet out of the bath and wipe them dry, and if they are not warm to the hand, have them rubbed as the upper extremities: this treatment is to be continued until reaction takes place.

I prefer alcoholic stimulation to any other, but most of the writers speak against it, from a fear of a reaction being too powerful, though I have never seen any unpleasant symptoms arising from its use. As a general thing, reaction is very slow in the disease, and by the above treatment, soon after reaction commences, the patient is very apt to get into a moist perspiration. I have found the oil of turpentine and African pepper

both useful stimulants, internally as well as externally used. I have had no case in which the brain prevented the use of alcoholic stimulations.

As soon as reaction was fully commenced, we placed our patient on the sulphate of quinine, stopping in a measure the use of everything internally, giving it to him in broken doses, until his system comes fully under the influence,—giving him from twenty-five to sixty grains before the time for the return of the next paroxysm.

If the patient has taken thirty or forty grains of the sulphate of quinine, and the period for the next paroxysm is approximating, and the quinine has not affected his head, give him a drink of brandy; continue the brandy and quinine, and you need not fear a return of the paroxysm. I have not found much benefit from the use of quinine during the paroxysm; indeed, I have seen ill effects arise from its use, nauseating the stomach, and causing it to reject other medicines.

Pneumonia prevailed to some considerable extent last Fall and winter, and it continues to show itself in some parts of our county. The majority of the cases are typhoid, to which I shall call attention.

Typhoid pneumonia is most apt to make its appearance in constitutions worn down by other diseases, especially those patients that have been suffering with malarial diseases with depraved blood; or in old persons, whose vital forces are feeble, and in those whose continued daily labor is too much for their strength, not giving them time for their system to re-act.

The patient complains of feeling badly several days before his attack,—loss of appetite, dull and weak, with unpleasant sensations about his bowels. The attack comes on with a chilly feeling, which may last for several hours, with a thirst for cold water, which continues.

There is a peculiar odor emitted, which I believe almost pathognomonic of this disease, and may be detected as soon as a person approaches the patient. His tongue is generally coated over in the first stage of the disease with a white fur, though sometimes it is yellowish, with a dry streak several lines wide

in its centre. He complains of his mouth being dry and clammy,—pulse weak, frequent, compressible, and sometimes very small,—skin rough and dry. There is more than the usual amount of pain complained of, though there are some cases that do not complain at all.

The cough is very troublesome, dry, hacking, with difficulty of expectoration. The expectoration is much less, as a general thing, than in other forms of pneumonia,—not so much of the bloody sputum, though sometimes a case will present itself that has an excess of the secretion. The bowels are generally constipated in the first stages of this disease, but very readily acted on by the mildest medicine.

Treatment.—In the treatment of this disease, tonics and stimulants are indispensable, but great precision must be exercised in the administration. If the patient is suffering with pain in the side or breast, cup him,—cup him freely, if he can bear it; have his feet bathed in warm water and then wiped dry; open his bowels with a small dose of calomel, following it in six or eight hours with castor oil.

The most approved treatment with us is, to place your patient on calomel and ipecac. in small doses, to be given every two or three hours,—from a half grain to a a grain of calomel, and as much ipecac. as may be necessary. Neutral mixture comes in here well, as a febrifuge, and flaxseed tea as a drink, to assist in expectoration. The calomel and ipecac. should be continued until the gums are slightly affected, and then used so as to keep up gentle ptyalism, being careful that the mouth does not get very sore. If the pain continues, and you have used the cups as far as you think prudent, apply a large blister over the diseased lung; but care should be exercised that it does not remain on too long and cause suppuration. The blister may be re-applied as soon as it peels, if the pain continues. In the course of five or six days, you will find use for your tonics and stimulants; indeed, stimulants are required in some cases freely, at the commencement of the disease. Quinine is preferred to any other tonic, and may be combined with the calomel and ipecac.

The patient's strength must be kept up, and as soon as it commences to decline, he must be placed on quinine and brandy. Milk-punch is a most excellent article to put your patient on, given to him as he may require it.

This is a disease in which the bowels are very difficult to control, and I believe many cases have proved fatal from the bowels becoming incontrollable. Opium, acetate of lead and ipecac., should be given, but not carried too far.

In the course of the disease the bowels may break loose, and the opium and acetate of lead does not check them, but appears to aggravate the case; and so long as you continue the mercury the discharges increase: it matters not what form of mercury you use, or whether it be internally or externally employed, the same effects are seen upon the bowels. The tongue becomes dry, cracked, and sometimes bleeds.

If you will examine the discharges from the bowels, you will find them first thin, mixed with a slimy mucus—the amount of mucus increases with little strings of blood. If the mercurial is continued, the discharge becomes very thin and watery, looking something like healthy urine in color, but not so clear,—the red color is from the amount of blood it contains.

The discharge is very offensive, and I think not very unlike the odor coming from a patient suffering with ptyalism. If you will examine the abdomen, you will find tympanites. Here you have a case of mercurial enteritis; your patient under the influence of mercury, and the salivary glands not acting,—indeed, almost, if not entirely, suspended in their functions. I can see a very conclusive reason why a physician might be led to an error: having great anxiety for his patient, and believing he has a common case of irritation of the bowels,—his whole attention drawn to the lungs, and having unbounded confidence in the mercurial treatment,—he is likely to change the form and continue the treatment.

As soon as you detect this peculiar odor, and see slimy mucus discharges, with little strings of blood, cease the use of mercury. Continue your tonics and stimulants,—give him oil of turpentine in mucilage, six or eight drops every four hours.

Try the opium and acetate of lead to check the bowels and quiet the patient; but should they fail to check them, place him on the nitrate of silver,—the fourth of a grain every four hours,—which will check the action of the bowels when nothing else in my reach will. Under this treatment you will be gratified soon, by seeing the tongue become moist, the action of the bowels diminished, and you will be very apt to have a case of ptyalism.

Very soon after the system is affected by the mercury, whether shown by the mouth or intestines, the lungs appear to improve, as well as all other symptoms, though sometimes the fever continues, and runs its course, wearing itself out, or exhausting the patient.

The Measles prevailed quite extensively in some parts of our county, last spring and summer, but presented nothing new in its character.

Scarlatina made its appearance in this county about the first of January, in a mild form, and continued mild in its character until the middle of February, when it became more aggravated, and finally very malignant. In some one or two families it was very deadly,—nearly half the cases that were attacked proved fatal, while in other families the cases took on a milder type, and did well.

The most of the cases that proved fatal perished before the disease had developed itself. They would be taken and die in the course of twenty or thirty hours, from the overwhelming force of the shock upon the system.

I noticed that almost every case that assumed the malignant variety suffered very much in the first stage of the disease, with nausea of the stomach and vomiting, attended with considerable pain, while the other varieties suffered but little, if any, with nausea or vomiting. The treatment of this disease depends very much upon the variety which presents itself: if it be a mild form of the disease, it does not require much treatment. A small dose of Epsom salts may answer every purpose, with a light diet. But if the patient has quite a high fever, with some irritation or inflammation of the throat, and has some nausea of the stomach, give him a gentle emetic of ipecac. and warm

water, and as soon as the effects of the emetic passes off, give him a small dose of calomel, following it, in six hours, with Epsom salts or castor oil,—having his feet soaked in hot water as soon as he takes the calomel. Put him on neutral mixture,—a table-spoonful once in two hours; or, the chlorate of potassa, which I like very well as a drink, dissolving a drachm in a pint of water. He should be drinking pepper tea all the while, and not allowed to take the wind. He should be using a gargle of sage-tea and honey, with enough alum in it to make it rough, three or four times a day. He may drink cold water in small quantities, to quiet his thirst, as often as he may want it.

The above treatment may be all that is required; but the insidious character of scarlet fever makes it necessary that the physician carefully watch its progress, as alarming symptoms may present themselves at any moment.

Should the Anginose variety present itself, which is characterized by the disease concentrating itself upon the throat, you have to direct your attention to that organ. In this form of the disease the rash makes its appearance a little later, and does not often spread over the whole surface of the body: sometimes it is seen only in little patches on the neck or breast, or hands and arms, or face; or it may be feebly diffused over either of those parts.

Should the inflammation of the fauces become very troublesome, deglutition difficult, or signs in the fauces of pseudo-membranus patches, mop the whole fauces with a strong solution of nitrate of silver—from ten to forty grains to the fluid ounce of water,—once a day. If ulcers are seen in any part of the throat, you may apply the nitrate of silver itself to the part occasionally. The gargle of sage-tea and honey should be used often in this variety. The throat should be rubbed externally with oil of turpentine and sweet oil, three times a day. Should the symptoms continue without any improvement, apply a small blister to the throat, and let it remain only long enough to draw a few little vesicles, and then use an emollient poultice. This will answer every purpose, and give your patient a great deal of ease. I have never seen any ill effects

arising from the use of blisters, carefully attended to, and not allowed to remain on too long.

Should a case present itself with very frequent, feeble and small pulse, anxious expression about the countenance, restlessness—turning from one side of the bed to the other,—tip of the fingers, nose and feet cold, with a pale face, with nausea or vomiting, with a scarlet-rash feebly making its appearance, or flushing out and then receding, you may be sure you have the malignant variety to contend with, though some of these symptoms may not necessarily present themselves.

Here you have a case wherein the disease is overwhelming the vital forces of the system, which, without assistance, will soon be destroyed. Internal stimulation is good, but it will not answer the entire purpose. External stimulation must also be introduced.

I will read a report of a case, giving the only successful treatment which I have been able to obtain. This treatment I have had an opportunity of testing, and have more confidence in it than in all others:

A boy, six years old, was taken on the 12th of March, about midnight, with vomiting. I saw him the morning of the 13th. He was quite restless, with some signs of a scarlet rash making its appearance on his body; his pulse quite frequent and small; his tongue coated over with a white fur in its centre, and the tip and edges red with papilla. One of his hands and arm very much enlarged with erysipelas.

I gave him an emetic of ipecac. and warm water, and as soon as the effects of the emetic was over, I had him put in a hot foot-bath, and gave him a small dose of calomel, following it in six hours with castor oil.

His hands and arm was kept covered over with sweet oil, and pepper-tea was given to him as a drink.

14*th.*—In the morning pulse very frequent and small—rash more developed and of a deeper color. His skin dry—mouth dry and clammy—delirious. Erysipelas extending towards his body.

I put him in a bath for a little while, then had him wiped dry, and gave him pepper-tea—neutral mixture. He soon be-

came moist and quiet. I gave orders, should his skin become dry and the patient restless, that he should be placed in a bath, or sponged with hot water. *Evening:* I was called in haste; the patient was having convulsion after convulsion; pulse 168—skin dry—rash receding—tip of his fingers and nose cold.

I put him in a hot bath to his arm-pits, and kept him there a few minutes; then took him out and wrapped his body and lower extremities in a flannel wrapper, wet in hot water and mustard, about the consistency of milk. He was given pepper-tea. The little fellow soon became quiet and in a perspiration, with a deep scarlet rash covering the whole of his body. I removed the flannel wrapper, and in its place put a dry one, and continued the pepper-tea and neutral mixture. His skin was kept moist by the means of the bath and flannel wrapper.

15*th.*—The little patient better—skin moist—pulse 136—rational. The rash is all over him, of a deep scarlet color. The erysipelas blending with the rash, and his body, and down his back, of a dark scarlet color.

The neutral mixture and pepper-tea continued, with orders to use the bath and flannels, if necessary.

16*th*—*Morning:* Patient doing well—pulse 126—skin moist—tongue a little moist, and cleaning off, leaving it very red and sore to the touch. Rash still all over the body, but the color is not so deep; bowels have been good all the while,—kept open with Epsom salts, in small doses.

Treatment carried out as directed—the wrapper used once.

17*th.*—Patient continues to do well—pulse 125—skin moist—eruption paler—erysipelas disappearing—slight retention of urine. Put on the chlorate of potassa instead of the neutral mixture. He wants to eat, and has given him tea and cracker.

18*th.*—Doing well; desquamation of the skin commencing about his hands and neck. Put on tonics, and continues to do well.

I believe in malignant cases, where the vital forces are so depressed, it is better to omit the emetics.

To prevent sequelæ, and to build your patient up, place him on a course of tonics; as soon as the fever commences to decline

and desquamation shows itself, use the tincture of muriate of iron and quinine.

VERATRUM VIRIDE.

The patient in this case was my own child, a girl just two years old, who had been all her life remarkably healthy and vigorous. On Friday, the 17th of September, 1858, she was attacked with chill, early in the morning. The fever passed off in the evening, and on Saturday morning she was as well as usual. Believing it to be only a common case of intermittent fever, I was in no hurry to give her medicine, intending to give quinine on Saturday. Before I had commenced to do so, however, another chill came on. Fever followed, and towards night was very high, the skin continuing very hot and dry. I administered a dose of calomel, which operated next day, after oil had been given. On Sunday there was no abatement of the fever, and she began to be drowsy, and to start in her sleep. During the day I gave her the warm bath, nitre, ipecac., warm teas, fomentations and various other means, to bring about sweating, but I could perceive no moisture of the skin and no abatement of the fever. On Sunday night my fears were alarmed. At 12 P. M. her pulse was 140; skin intensely hot and dry; considerable drowsiness; lips and tongue becoming very dry, and sordes collecting on the teeth. The respiration was 40. Under these circumstances, after having failed to check the disease with the usual remedies, I determined to try the veratrum. It seemed to me to be a suitable case to test its action. I had long desired to try the medicine, but had waited for a case in which I might constantly watch its effects. At 12 P. M. I gave the first dose, using Norwood's tincture, and mixed according to his directions, viz.: one drop of the tincture to two drops of syrup scillæ. I commenced with three drops, intending to repeat the dose every three hours. At the second dose, gave five drops; at the third, gave six; at the fourth, gave seven. I discovered no applicable effect until after the fourth dose. Then I noticed a decline

of the fever. The skin was becoming moist, and softer and cooler. The pulse, too, had sensibly diminished in frequency. At this time one of my brother physicians saw the case, and judging from the favorable action of the medicine so far, its further use was advised. So, at the proper time, another dose of seven drops was given, and, satisfied with her favorable condition, I left home to visit another patient. This last dose proved too much for her. About an hour after its administration nausea and vomiting set in, and so alarmingly were the symptoms that I was sent for. On my arrival I found her vomiting almost constantly, a glairy, greenish, viscid fluid, with which the bowels and stomach seemed filled. There was constant gurgling of fluids in the intestines. The skin was cool, the face very pale, and bore the expression of great distress. The respiration seemed at times almost suspended, and so long did she lie without breathing, that I constantly shook her, without which I think she would have ceased to breathe. Pulse was now about fifty. The lungs, which had shown a healthy condition under auscultation, now seemed filled with a secretion of some kind, requiring occasional cough to eject it from them. The discharges from the bowels were frequent, and were exactly of the nature of the matter vomited, mentioned above. The prostration was alarming indeed. I immediately administered the antidote recommended by Doctor Norwood, viz.: morphia and essence of ginger, with sinapisms to extremities, &c. In about one hour from their supervention, these symptoms began to abate. The capillary circulation was restored, the skin became warm, the countenance lost the expression of anxiety, the pulse rose to eighty, and the patient appeared in a better condition than since the attack. The last dose had been given at 12 M. on Monday. The antidote was given at 2 P. M., and at 4 P. M. she was in the favorable condition just described, and which continued until 10 P. M., inducing me to believe that the disease was broken up, and the patient convalescent. She slept most of the time, but answered questions rationally when addressed, seeming to be unwilling to be disturbed, and preferring to be allowed to sleep. She was perfectly under the influence of the morphia

given to counteract the effects of the veratrum. My hopes of the patient's convalescence were not realized, however. After 10 P. M., I noticed a return of the fever, with restlessness, and an increase in the frequency of the pulse. By 12 P. M. the pulse had reached 100; the skin hot and dry, and the patient seemed to be rapidly going into the condition described before commencing the veratrum. Favorably impressed with the sedative powers of the medicine, notwithstanding the powerfully prostrating effects which I had noticed, but which I believed might be prevented by cautious administration, I gave another dose of five drops of the mixture, determining to watch closely for the first effects, and to administer the antidote on the first appearance of nausea. In about an hour from the administration of the medicine, its effects were shown by restlessness, paleness of the countenance and nausea, and notwithstanding the antidote was given promptly, the symptoms of poisoning went on to a most fearful extent. The vomiting and prostration were more excessive than at first, and the respirations were sometimes not more than two per minute, and but for constant rousing, must have ceased entirely. The same fluid was vomited and discharged by the bowels as before, and the lungs, too, again seemed suddenly filled with a secretion causing loud râles all over the chest. I gave ginger and morphia, paregoric and brandy; applied sinapisms, and used every means I could think of to stimulate the system, but for two hours every effort seemed in vain. In about this time there was perceptible a slight amendment, and in a short time all the alarming symptoms had passed off, except the stupor, which did not diminish at all. The pulse, which had been below fifty, rose to a natural standard; the skin became warm, the breathing natural. The stupor, however, continued through the day, the pupils being intensely contracted, and I was convinced that the patient was laboring under the effects of the morphine given to counteract the veratrum. In the course of twenty-four hours the narcotism passed away, though it had been alarming, and from that time convalescence was established.

There are one or two points in the case to which I wish to

call particular attention. The patient was drowsy from the commencement of the fever. She had once before had convulsions during a fever following a chill, and had always been drowsy when she had a fever. Still there was no decided stupor about the brain until the veratrum had been given. Under the prostrating effects of this medicine there was decided stupor, it being impossible to rouse her only by much shaking. This, however, passed off as the effects of the veratrum passed away. When the poisonous effects of the veratrum came on a second time, narcotism returned also. There was no difference in the effects of the veratrum and of the morphia, nor was it possible to tell, by the narcotism alone, when the veratrum ceased, and the morphia began to act. I believe the narcotic effects of the veratrum to be much more transitory than those of morphia.

I have already spoken of the peculiar matter vomited, and discharged by the bowels. This I believe to have been suddenly and abundantly secreted, through the effects of the veratrum, as it was only present during the prostrating effects of the medicine.

The veratrum seemed to have had a specific action on the lungs. Until the bad effects of the veratrum were shown, the lungs, under a careful examination, gave no indications of disease whatever. Under the effects of the medicine, however, they seemed to be suddenly filled with mucus, or secretion of some kind, all traces of which disappeared when its effects passed off; and on the second supervention of untoward symptoms, they were again filled with fluid. Whatever may have been the cause of the secretion, it was transitory, no sign of disease remaining after the effects of medicine passed off.

As to whether the veratrum broke up the fever or not, I cannot say. It had raged for forty hours without any abatement, and did not return to much extent after the last dose of the medicine.

The following case will show, not only the powerful effects of the veratrum in reducing arterial action, but also in subduing active inflammation.

Joe, a colored boy, about sixteen years old, had been subject toRheumatism of the joints for a year or two, causing occasional confinement. I first saw him on Thursday, the 12th of May last. He had been attacked the day before, while at work in the field, with a chill, as his master supposed. I found him lying upon his back, breathing very rapidly, and with much difficulty, having a drowsy look, and groaning heavily. He complained of much pain in left side, over the heart. The pulse was 160. On applying the ear to the back of the chest, the peculiar crepitus of pneumonia was heard all over the left lung. Fearing, from his having had rheumatism previously, that the heart was affected also, I endeavored to satisfy myself, by auscultation of that organ; but the pulsations were so frequent and tumultuous (resembling the action of the heart in hypertrophy), that I could make no correct diagnosis. The skin was very hot and dry; tongue heavily coated with a yellowish fur, but moist; bowels costive. I gave him an active dose of cathartic medicine, and commenced giving tartar emetic in nauseating doses: ordered a hot foot-bath at night. My next visit was on the morning of the 13th. I found the patient in about the same condition as at the first visit. The cathartic had acted well on the bowels. The tartar emetic had produced nausea and vomiting, without any relief to pain, or without altering the state of the skin. The pulse 160, with dyspnœa; drowsiness and cough unchanged. I ordered the tartar emetic to be increased in quantity, until it produced purging, or till the symptoms just detailed gave way to its influence; also, Dover's powders, in small doses, to act on the skin, fomentations to seat of pain, and castor oil to keep the bowels open.

Visited him again on the 14th, and found no improvement. The tartar emetic, having produced purging, was discontinued during the night. Pulse and skin about the same. He had been delirious through the night. Percussion revealed dullness all over the left lung, and auscultation showed bronchial respiration. Tongue dry, and there is much drowsiness, with muttering in sleep. I considered his condition much more unfavorable than it had previously been. I determined to try the effects of

the veratrum, as the treatment seemed to have no effect on the disease. I made a mixture of one teaspoonful of Norwood's tincture, with twelve of hive syrup, and commenced with forty drops of the mixture. Three hours afterwards I gave sixty drops, then ordered the dose increased ten drops every three hours, until the desired effects were produced. Only a few doses were given before the skin became moist, the breathing more easy, and a decided amelioration of all the symptoms took place. The pulse was reduced to 100 during the night.

I had thought it proper to warn the mistress of the boy in regard to the effects of the veratrum, and to give directions for administering the antidote, in case it acted too violently. Fearing the bad results of an over-dose, and satisfied with the change already produced, she would give no more, after reducing the pulse as above named. In this condition I found him on the 18th. All other medicines had been discontinued. I now gave an expectorant mixture and applied a blister over the seat of the pain. It is needless to follow the case further. By inducing his mistress to give the medicine freely enough, the pulse was kept below 100, and the skin cool and moist. Soon the pneumonic crepitus was heard again, and the lung retraced its course in a way beautiful to notice. The convalescence was rapid, and on the 19th the patient was discharged. G.

Rings from Swollen Fingers.—The mode of proceeding is this: a reel of cotton is wound evenly round, beginning on the extremity of the finger, and bringing each coil into close apposition with the preceding, until the ring is reached. A needle is then threaded with the cotton, and passed under the ring, and the thread is carefully unwound from the finger. The ring follows each coil, as it is successively unrolled; and, by almost imperceptible degrees, is brought over the knuckle, and removed. Care must be taken that the cotton is wound on evenly, or an entanglement will occur in the unwinding. A small curved needle will pass more rapidly under the ring than a straight one.—*British Med. Jour.*

Bibliographical Notices.

Clinical Lectures on the Diseases of Women and Children. *By* Gunning S. Bedford. *A.M., M.D., Professor of Obstetrics, the Diseases of Women and Children, and Clinical Midwifery in the University of New-York.* S. S. & W. Wood. 1859.

This is a rich offering to the shrine of medical science. It has been well received by the profession. Dr. Bedford has a national reputation, not only for affluence in thought and eloquence in speech in the lecture-room, but is widely known as one of the most accomplished and skillful practitioners in the United States. He is a great favorite with the large classes that flock to the University of New York every session; he is remarkably successful in making contagious his own noble enthusiasm for his particular department; and the past and present prosperous condition of the University is owing in a great degree to his attractiveness as a Professor; although such men as Paine, Van Buren, Mott and Draper are well calculated to give reputation to *any* school.

In whatever pertains to the womb—in everything relating to the numerous and complicated diseases of this important viscus, he has not, in all probability, a superior in the country. He has devoted much time and attention to investigating its diseases—now largely on the increase in the United States,—and the profession are indebted to him for what he has done, especially for his improvement in the diagnosis of uterine affections, in which he is one of the pioneers. His efforts have been crowned with signal success in this department. Though comparatively a young man, he is rapidly rising to a proud and enviable position in the first ranks of his profession. His genius and professional devotion have long since marked him as a man of distinction and of progress. His enemies—and most good and useful men have them—say he is too ambitious; if he be ambitious, we feel assured that his ambition is not grovelling or unworthy, but only such as is connected with the improvement of medical science, the exaltation of his profession, and the honor of his country.

The book comprising these lectures has only been out two or three years, and yet five editions have already been exhausted. This fact speaks more for the author than anything we could say. He has enjoyed the finest opportunities for producing such a work. He discusses practical questions, and discourses on every-day diseases, and does it in such a plain, direct, attractive way, that the practitioner will find the book interesting and valuable. It has been objected that there has been too much of the catechetical order observed in these lectures. We differ with such objectors. The philosophy of the human mind, and the common-sense views that prevail in our best systems of instruction, recognize the method pursued by Dr. Bedford, as the best, and the one most likely to impart and impress information of a practical character. He who has once heard Dr. Bedford never *forgets* him; he who reads his books will *remember* his wholesome truths conveyed in his own peculiar, eloquent and touching style.

S. S. S.

Fifth Annual Report to the Legislature of South Carolina, *Relating to the Registry and Returns of Births, Marriages and Deaths in the State, for the Year ending December* 31*st*, 1858. *By* R. W. Gibbes, *Jr.*, *M.D.*, *Registrar, Columbia, So. Ca.* 1859.

We are indebted to the kindness of a friend for the above Report, which we have examined with much attention and pleasure. It is a source of pride to remember, that in the great work of collecting statistical information in regard to this most important subject, a sister and a Southern State has set North Carolina an example which she will doubtless delight to follow, just so soon as the popular mind is prepared for the step. It is useless, in this age of enlightenment, to discuss the advantages which must accrue to the human family by the faithful collection and classification of such important facts as are embodied in this Report; and we will conclude this notice by an expression of our great respect for the talent and industry displayed by Dr. Gibbes, and an earnest appeal to the Profession of this State in behalf of such measures as will ensure a similar registration in North Carolina.

The Subjective and Objective Influences of Medicine. *By* E. B. Haskins, *M.D., Nashville, Tenn.* 1859.

This very eloquent and able address was delivered at the opening of Shelby Medical College, in October last; and we hereby tender our thanks to its author for the copy sent to us. We had the pleasure of meeting Dr. Haskins at Louisville last year, and know him to be a high-toned, chivalrous gentleman, as well as a ripe and finished scholar. This address of his proves conclusively, that he can defend the principles of our noble science with as much success as crowned his rebuke of impertinent effrontery on another occasion.

Botany as an Ally to Medicine. *By* George S. Blackie, *of the University of Nashville, Tenn.*

This "noble plea for the study of Botany" was delivered before the Medical Society of Nashville, which body, appreciating its rare beauty and excellence, has published it for the benefit of the profession generally. Had we space, it would afford us pleasure to give it to our readers *in extenso;* but will content ourselves for the present by saying, that it is a masterly production, well calculated to benefit the Profession, and to reflect honor on its author.

Proceedings and Debates of the Third National Quarantine and Sanitary Convention, *held in the City of New York, April*, 1859. New York, 1859.

We are under obligations to Dr. John H. Griscom, President of the Convention, for a copy of this work, which we are pleased to see has been printed by the Common Council of New York, in a very complete and handsome manner. This Convention was the largest of its kind, and certainly the most important, that ever assembled in this country. Composed of delegates from every section,—embodying the views of those who were presumed to be well acquainted with the subject,—assembling in New York, the great scientific and commercial emporium of the land,—its proceedings attracted the most marked and universal attention; and the volume now before us, issued as it is under the immediate auspices of this great body, as the author-

ized exponent of its views and acts, goes out to the world invested with a *prestige* which will insure it an attentive, but at the same time a most critical examination. Desirous of fully understanding the whole subject of Quarantine, and anxious to do this Convention the fullest justice, we have read this volume carefully; and, as a public journalist, we propose to give our candid and deliberate opinions respecting the "Proceedings and Debates" of this august body. Following the order of the book itself, we shall begin with the debates,—if the rancorous, unphilosophical and illogical wrangle indulged in by many of the speakers can be dignified by such a name. New York is the commercial centre of the country. There, Trade is king. There, everything is made subservient to the great master-passion for making money. There, dollars and cents are regarded as the measure and exponent of a nation's glory. And as quarantine regulations must paralize trade in a measure,—as they prevent, to a certain extent, the acquisition of money,—as they are based upon the broad and comprehensive truth, that population, rather than dollars and cents, tends to increase the permanent prosperity of a State, they are necessarily unpopular in that city. The merchant princes, the millionaires, the great admirals of that "grand metropolis," believe that their interests are materially injured by whatever restrains its commerce; and as they constitute the most powerful and influential class, they give tone and character to public opinion in this matter, as well as in many others of less importance. Accepting their impressions for facts,—falling naturally into the crude and contracted views entertained by these interested parties, the community, or a large majority of it, has gradually been convinced of the uselessness of quarantine restrictions, and of the importance of a radical change in them,—at least so far as relates to the city of New York. Even the most honest and philosophic of her professional men, have yielded to the extraneous influences surrounding them, and with that fatuity which has attested the fallibility of the human intellect in every phase of its eventful history, they have been seduced, by their own wishes, into positions and opinions which an unbiassed judgment would have pronounced

heretical and heterodox; for, let the fact be disguised by optimists as they choose, human reason has ever been under the dominion of human passion,, and must remain so until the curse of Adam is removed from his race. Men will believe what they wish—what is agreeable to them,—what their interests or their prejudices demand. This may be affirmed of the wisest and the best of the human family; and whilst it establishes nothing against the integrity of those whose acts demonstrate its truth, it shows conclusively that man, with all his boasted strength of intellect, is but the creature of circumstances which surround him.

The delegates to this Convention from other sections found this state of things existing in New York, on their arrival there, and being generally of impressible material, because themselves the representatives of similar interests, they yielded to such influences as we have described, and were speedily convinced of the necessity for reform in the quarantine regulations of the country. They inhaled this opinion in the savory odors of the magnificent feasts spread for their entertainment; they drank it in with the foam of the sparkling champagne; they breathed it in the fragrant cloud of their royal Havanas; they saw it emblazoned on the towering halls of the marble palaces whose portals opened for their reception; they heard it amid the rustling sails and waving pennants of the gallant ships which saluted their coming; and they finally became so infected with it, as really to esteem it, not only an appropriate offering to the hospitality of their liberal hosts, but even as a badge of respectability, and an evidence of their acquaintance with the subject of quarantine in all of its phases and details.

Impressed by feelings and sentiments of this character, a large majority of those who participated in the debate, seemed to regard the questions which have agitated and divided the Profession for years, as settled beyond all controversy, whilst they believed that the "Quarantine Convention" of 1859 was especially commissioned to announce the fact, and to establish a great sanitary system which should command the admiration of the world, and give especial satisfaction to the city of New York. Hence

it was that the disputed points in regard to the introduction of yellow fever into the West India Islands, &c., were as summarily disposed of as if there had never been any controversy on the subject. Hence it was that the history of the various epidemics was given as glibly as if the narrator alone, of all men, had witnessed their progress; and hence the confidence with which even the most inexperienced, discoursed of the etiology, pathology, and essential nature of certain zymatics, about which the most learned and laborious observers are hopelessly divided.

Special pleading is only a logical subterfuge. It is the artful dodge of an adroit tactician who distrusts the strength of his cause, or his own knowledge of its merits. It is what cunning is to manliness,—what adroitness is to real power,—what strategy is to the regular and open attack. It is the attempt to accomplish by subtlety that which straightforwardness could never effect,—to secure a triumph through the instrumentality of an artifice, rather than by legitimate and honorable means,—to overwhelm an adversary, not by meeting him on fair and open ground, but by enticing him into an ambush, and attacking him at an unguarded point; and hence it is in itself a confession either of the inherent weakness of the cause in which it is employed, or of the ignorance of the advocate himself.

We have been particularly assured of the truth of these propositions, by a careful perusal of the speeches made in support of the reforms to which we have referred; and we feel confident that the sophistry which characterizes them, will be universally esteemed an enduring proof of the unsoundness of the doctrines advocated, or of the weakness of their defenders.

The fact that the names of such men as Griscom, La Roche, Wood, and others of equal reputation, appear among the advocates of these Utopian views, is conclusive evidence that no charge of intellectual weakness can be preferred against them; and hence we must infer that the cause itself was not susceptible of an abler defence.

In strong contrast with the special pleadings of the non-contagionists, stands out the pertinent, logical and most elo-

quent remarks of Dr. Francis,—that worthy representative of a past and purer generation. The very straightforwardness of his manner, together with the simplicity of his style, and the clearness and precision of his statements, makes out a *prima facie* case in support of his opinions. And we must say that, in our judgment, though opposed by the ablest men of the country,—by those who had made the subject their special study,—by all the eloquence, learning and subtlety the occasion could command, his defence was masterly and his triumph complete.

Of the proceedings of the Convention we shall say but little more than is necessary to make them understood by our readers. After a long discussion, to which we have just referred, a vote was taken on the following resolution, which resulted in its almost unanimous adoption:

Resolved, That, in the absence of any evidence establishing the conclusion that yellow fever has ever been conveyed by one person to another, it is the opinion of this Convention, that the personal quarantine of cases of yellow fever may be safely abolished, provided that *fomites* of any kind be rigidly restricted.

It will be perceived that the Convention has summarily disposed of the great question of the contagiousness of yellow fever, and that from henceforth the whole matter is to be considered definitely settled. Hereafter no man is presumed to think for himself on the subject. Hereafter the ablest observers of the disease, in this and every other country, are to abandon their opinions, to reject the evidences of their senses, and submit to the doctrine of this august body; although many of its members had never seen a case of yellow fever, and a large majority were influenced by the indirect but most positive instructions of their constituents. They deliberately rejected the evidence of such a man as Dickson, of Philadelphia, who asserts in the most imperative manner that its contagious character has manifested itself not only under his own immediate observation, but in the experience of other medical men of the highest respectability, and that he is being supplied with additional proofs of its communicability every year. They discredited the testimony of Pyn, Blanc, Wistar, Hosack, Varhé, Monette, Strobel, Fenner and Francis, — all able and reliable men. They repudiated

the recorded and authenticated facts in regard to the island of Boa Vista, of the coast of Bulam, of the ships Hankey, L'Eclair and Mandarin, and of the epidemics of New Orleans, Philadelphia, Charleston and Norfolk, without hesitation or explanation. And the only solution of the mystery which attaches itself to the process by which their extraordinary conclusion was reached, is the one which we have previously shadowed forth in the declaration, that interest and prejudice are more powerful magnets than truth and justice, and that human judgment acknowledges their influence in obedience to an unalterable law of its nature.

But the strangest part of the performance has yet to be mentioned, and we record it under the conviction that there was a clerical error at the bottom of it originally—though some of the expounders of the resolution seem disposed to stand up for it in its totality,—contradictions, absurdities and all. They repudiate the doctrine of the personal contagion of yellow fever, and admit that it may be propagated through the instrumentality of *fomites*. Now, it is not necessary to examine authorities for the purpose of ascertaining the precise meaning of the term *fomites;* for even it were not well understood by the Profession, those who voted for the resolution have given an authoritative explanation of it, which is a part of the record of the Convention. Dr. Watson, one of the glorious Eighty-five, says—"We mean, by *fomites*, those materials, whether inorganic, animal or vegetable, by which the mortific agent, or virus, is carried from one place to another. I believe it is *generally admitted* in the medical profession, that goods, bundles of hair, woolen clothing, cotton bales, and other substances, may act as *fomites* for the spread of the disease." And Dr. Stevens, the very mover of the resolution, and its acknowledged expounder, uses the following language: "Dr. Watson's sentiments and mine are the same." Here, then, we have an authenticated definition of *fomites;* and hence there can be no doubt as to the meaning of the Convention in this connection.

Accepting this definition, it follows necessarily—as Dr. Watson himself saw, but failed to appreciate,—that the human system comes within its pale, and that it is impossible, by any species of special pleading, to suppress the fact.

But, admitting for the sake of argument, that the human system does not belong to this category, and that it cannot transmit the morbific agent or virus by which the disease is produced, the resolution still contradicts itself, and stultifies its supporters. *In the first place*, it assumes, that those actually suffering with the disease cannot communicate it directly to other persons, under any variety of circumstances, but that they can communicate it to certain *things*, such as bundles of hair, woolen goods, &c., which in their turn are capable of infecting the human system. Now, as it is not pretended that these *fomites* are capable of modifying or even intensifying the virus communicated to them, but are simply the passive instruments of its transmission,—and as the fact of their impregnation by the bodies of *persons* affected with the disease, shows that there is some morbific effluvia evolved under these circumstances, it must follow that yellow fever generates a poison which does impress the human system deleteriously when brought in *contact with it*, and that the disease is personally contagious. In a word, if these fometic agents effect no specific change in the poison, and are not *necessary* to its conveyance, *i. e.* to bringing it in contact with the human system, then the conclusion is inevitable that yellow fever is *contagious* in the strictest, as well as the fullest sense of the term. Besides, even it does communicate itself to *things* and not to *persons*, and is thus indirectly reproduced, is it not the merest quibble that even sensible men amused themselves with,—to deny the fact of its *communicability*, and to demand the abolition of quarantine? Would it be esteemed wise or "scientific" to block up the *trench* wherein gunpowder had been placed for the destruction of our property, and then to throw a blazing keg of it among our valuables? Would anything but the most arrant folly insist on breaking the innocent vessel which contained the deadly poison, and then drinking down the poison itself? Would any man, in his senses, think of driving away the horse which bore the robber to his door, and then letting in the robber himself? And yet, the friends of "the resolution" have been guilty of that which is equally as unwise, inconsistent and absurd.

In *the second place*, if the human body be not a fomite, the woolen coat, the cotton shirt, and indeed all the clothing which invests it, are *fomites*, according to the definition of the Convention itself; and more than this, the very hair upon the head, beard upon the chin, &c., belong to the same category; so that, in order to carry out "the resolution" according to its own provisions, these abolishers of "personal quarantine in cases of yellow fever",—these letters-in of *persons* and prohibitors of *things*, stand pledged to the world to admit every one who arrives from an infected district absolutely naked, and not only with—

> "No hair 'pon the top of his head,
> In the place where the wool ought to grow,"

but with every *patch* of that appendage carefully removed from the entire body. What a harvest for the barbers of New York! What a significant and grateful recognition of the association which once subsisted between those shavers of beards and the followers of Æsculapius! And what *sheer* nonsense this great *shave* is, after all the *soft-soaping* to which the honorable delegates were subjected in the great metropolis! One thing is very sure,—at least we will hazard our reputation as a prophet on it,—and that is, though the immortal "Eighty-four" may insist on introducing infected persons in a state of nudity, they will never introduce them into a State of this Union, Doctor Stevens' "vote of triumph" to the contrary notwithstanding.

But to be more serious, if this admitting *persons* and excluding *things*,—this bringing in the individual and leaving out both the natural and necessary covering of his body,—this abolishing "personal quarantine" and "prohibiting fomites," be not a palpable and inexplicable contradiction, then language means nothing, and every principle of our noble science is a fallacy.

Much that we have said respecting the debates of the Convention, will apply to the report on quarantine, which that body accepted and incorporated into its proceedings. We feel it to be an act of justice to add that, though its reasoning be false and its conclusions incorrect, there is much learning and ability

manifested therein, and that, both as a literary and controversial paper, it reflects much credit upon the committee.

In conclusion, we must say that, unless human testimony be utterly unreliable, there is positive and overwhelming evidence that yellow fever has been propagated by contact; and hence, we feel bound to dissent from the decision of the Convention, that personal quarantine should be abolished. Space will not permit us to enter into such a defence of quarantine as our feelings would suggest and our judgment sanction, but we cannot refrain from making one appeal in its behalf. The question is simply this: Shall graves or coffers be filled? Shall a nation such as ours, in the middle of the nineteenth century,—with the full blaze of an enlightened science streaming upon her,—standing upon the vantage ground of a pure and ennobling religion, and with all the startling lessons of experience appealing in their silent but most significant eloquence, to her,—shall she illustrate her venal scepticism, or criminal weakness, by sacrificing upon the altar of mammon one of the richest legacies from the wisdom and the humanity of the past? What if Commerce suffers and Trade is paralized? What if the Merchant Prince languishes in his stately palace, or eats the hard-earned bread of toil once more? Is not human life protected and suffering prevented? Are not those "who constitute the State", and add to her prosperity and her glory, preserved from "the pestilence that walketh at noonday"? Is not the fountain of tears in many a loving heart kept with its seal unbroken, and its waters undisturbed? And in the name, then, of humanity and religion, let us ask, if the compensation be not adequate, and the reward sufficient? Yes, they are, is the response of thousands; and thanks to our noble profession, the principles upon which the great system is founded are so firmly established, that, in despite of all opposition, they still loom up amid the proudest memorials of the past, and the noblest exponents of an advancing civilization.

SELECTIONS.

Practice.

[From the American Journal of the Medical Sciences.]

SPECIFIC CHEMICAL AND MICROSCOPICAL PHENOMENA OF GOUTY INFLAMMATION.

Dr. A. B. Garrod, in a paper read before the British Association at its late meeting, remarked that many and discordant views were held concerning the nature of gouty inflammation, and such diversity of opinion arose from the fact, that, up to the present time, no characteristic structural change had ever been demonstrated to accompany it. The object of his communication was to supply that deficiency, and prove that special chemical and microscopical phenomena invariably attend true gouty inflammation. After alluding very briefly to the views held by the ancients, and within the last century by Murray, Forbes, and Wollaston, and by Cullen and his followers, and of the difficulties which each had to contend with in applying their hypothesis to the explanation of the various symptoms of the diseases, he proceeded to speak of his discovery of the constant presence of uric acid in the blood in gout, and his subsequent researches in the nature of that disease. From these he first drew the three following conclusions: 1. In *health*, the blood contains minute traces of urate of soda and urea, and probably of all the principles destined for excretion; but the quantities are so small, that the most careful and refined analysis is required to demonstrate their presence. 2. In *gout*, the blood is invariably rich in urate of soda, and uric acid can be readily crystalized from it. 3. In by far the greater number of diseases the blood is free from an abnormal quantity of uric acid, but in certain cases of albuminuria, lead-poisoning, and other affections, its presence can be demonstrated, and still no gouty inflammation ensue. Lastly, in many gouty subjects the same condition exists in the intervals of the paroxysms. From these conclusions,

Dr. Garrod considered it evident that something more than the mere presence of urate of soda in the blood was required to produce gouty inflammation, and his next object was to ascertain its nature. For this purpose a careful examination of the joints which had suffered was required, and within the last few years many opportunities had fallen to his lot. The subjects of these examinations are divided into four classes: 1. Subjects of chronic gout with extensive chalkstones. 2. Subjects of gout with no appreciable deformity, and no visible deposits of chalkstones, except one or more specks on the external ear. 3. Subjects of gout in whom no trace of chalky matter was externally visible, and in one case only eight attacks of the disease had occurred. 4. Subjects in whom only a single joint (the ball of a great toe) had been affected with gouty inflammation, or in whom some joint had only been once slightly inflamed. These examinations proved beyond the possibility of doubt, that in the very slightest forms of the disease, as well as the most severe, a structural change invariably occurs, and that this change, when once produced, remained, if not permanently, at least for a very lengthened time. After detailing the microscopical and chemical characters of the deposit producing this change, Dr. Garrod finished his communication by stating that he considered the facts which had been brought forward warranted him to conclude that—"Specific, chemical and microscopical phenomena invariably accompany gouty inflammation, and these consist in the deposition of urate of soda in a crystaline form within the cartilages and ligamentous structures of the joints, and that such deposition is altogether pathognomonic, never being found in any disease other than true gout;" and again, that "Such deposition is probably the cause, rather than the effect, of the inflammatory action." Lastly, the author pointed out the great importance of ascertaining the true nature of the disease as a means of conducing to its rational and successful treatment.

TWO ESSENTIALLY DISTINCT CONDITIONS OF THE KIDNEY GIVING RISE TO WHAT IS CALLED BRIGHT'S DISEASE.

BY MR. DICKINSON.

[From the British Medical Journal.]

The results of the author's minute examination of the kidney under disease, lead him to the conclusion that the conditions, called after the name of Dr. Bright, are capable of arrangement into two great classes; that in the one we have disease proper to the secreting surface of the tubes, while in the other the disease is essentially intertubular, and that the characteristic difference between the two is the presence in the latter, and not in the former, of external granulation.

The "tubular disease" includes all the varieties of the large smooth kidney; notwithstanding that pathologists have subdivided it, according to the predominance of some one material, into "fatty degeneration," "waxy degeneration," "desquamative nephritis," and many others.

There are two distinct stages: First, the period of enlargement; secondly, that of diminution. In the first stage, the capsule is thin and unadherent; the cortex is much, the cones slightly, enlarged; there is no appearance of preternatural vascularity. Parts, indeed, may be bloodless, giving a mottled aspect. The microscope shows the tubes entire; but in the cortex they are choked up with their epithelial secretion, which is often in various stages of disintegration, or fatty degeneration. These are probably consequences rather than causes of the derangement, and result from the cells, formed in excess from the inflamed membrane, being of unhealthy composition. In the cones there is the same tendency to excess of epithelial secretion, but it finds a readier exit, and does not, therefore, so often accumulate there. The tubes here show rather a tendency to become filled with a glassy fibrinous material, studded with specks of oil, or cells of epithelium, which happen to be passing down, or blood corpuscles; and this is shed in the form of cylindrical casts. The casts, therefore, are formed almost exclusively in the straight tubes.

The second stage is rarely reached. It consists in an overstretching and bursting of the tubes one after another, those nearest the superficies being first destroyed; and it appears that their contents are then absorbed. The tubes collapse, and the organ may be reduced below its natural size. The capsule becomes thickened and a little more adherent. The surface is still smooth, the color unchanged. A section shows the gland to be chiefly composed of cone substance, and the remaining cortical substance is coarse, pale, and firm in texture, being reduced more or less to its fibrous nature; but there is no evidence of an increase of fibrous matter. The author agrees with Mr. Simon in believing that the cysts, often present in this condition, are developed out of epithelial germs, which are left on the walls of the ruptured tubules.

The "intertubular," or "granular disease," commences with slight thickening and adhesion of the capsule, with processes of fibrous-looking material passing from it into the interior of the gland; contraction of these follows, the points at which they originate become depressed, giving a superficial granulation to the exterior; and the tubes embedded in them, at first healthy, become gradually reduced to microscopic threads.

In what the author calls "chronic intertubular disease," there is generally diminution of size. The adherent capsule has a tendency to split into two layers; the inner one consisting apparently of fibrinous effusion. The surface is granulated, and fibrous processes pass from the capsule between the granules. The fibrous tissue of the cortical part is increased, and the tubulous waste. Some of the fibrinous material passes into the interior of the tubules, lining them and displacing the epithelium. Cysts are often present, and evidently result from transformation of the tubes, not, as in the tubular disease, from a change in the epithelium.

In the "acute intertubular disease," the granules are larger and more distinct, and there is more fibrinous exudation between and into the tubules.

The anatomical evidence in favor of the intertubular origin of the disease may be summed up as follows:—

1. Near the capsule an effusion is often seen obviously between the tubules.

2. Tubes are caused to shrivel by some influence which travels independently of their direction.

3. Tubes are sometimes divided into cysts, which can only be accomplished by a contractile force, external to themselves. In the smooth kidney this never occurs.

4. In most cases, some of the secreting tubes are found natural, as if they had been out of the way of the effusions; whereas, in the smooth kidney all are involved.

5. A decided difference exists in the contents of the tubes in well-marked cases of each sort. In one they are filled by their own morbid secretion, in the other they are occupied by a material which is foreign to it, and which is probably derived from without.

Of 357 fatal cases in St. George's Hospital, of which the particulars were collected by the author, 250 were granular, 107 smooth; of the latter the average age was 28 years, of the former 50 years; proving that the one disease is not a sequal or stage of the other. In no single instance has he known or read of the granular form resulting from scarlatina; and it rarely results from cold. Gouty or rheumatic diathesis was associated with the granular form in seventeen cases, with the smooth variety in only one, and that doubtful.

In examining the kidney, besides the ordinary mode of tearing the organ into shreds and fibres, it is the author's plan to prepare translucent sections, so as to show the tubes and Malpighian bodies in their relations with one another, and to the intertubular material.

A portion of the kidney is boiled in water, slightly acidulated with acetic acid. It is dried, and when hard a section can easily be cut from it, which, on the application of water, resumes its former bulk, and presents a beautiful picture of the anatomy of the kidney.

[Journal de Médecine et Chirurgie Pratiques.]

TROUSSEAU ON THE DIAGNOSIS AND TREATMENT OF HEPATIC COLIC.

Professor Trousseau lately directed the attention of his clinical class, at the Hotel Dieu, to the frequency with which cases of hepatic colic are mistaken for other affections. Although, in its severe form, hepatic colic is readily recognised, yet a slighter form, which is more common, especially in women, is very generally ignored. The reason is, that the pains caused by the small calculi are felt principally in the epigastrium, from which they radiate through the abdomen, the chest, the back, and sometimes even down the thighs. The practitioner, accordingly, is very apt to refer the pain to other organs than the liver, and, in particular, to ascribe it to a spasmodic affection of the stomach. This opinion appears, in many cases, to be further confirmed by the presence of vomiting.

M. Trousseau illustrated these observations by two cases, both women, of sedentary occupation, from forty to fifty years of age. In the case of the first, pain in the epigastrium, with vomiting of matter not containing bile, occurred two days before her admission into the hospital. The severe pain was succeeded by a feeling of languor and fatigue. The fœcal matters passed by this woman were collected, washed, and carefully examined, when a calculus of the size of a pea, consisting of cholesterine, was found. The second woman had been subject to "cramps of the stomach" for several years. These had been treated without success. When she came under M. Trousseau's care, she had been suffering for several days from attacks of severe pain starting from the epigastrium, and darting down through the belly into the right flank and into the back. These attacks came on two or three times a day, and lasted for from half an hour to two hours. On the 12th of March the pains continued for five hours, on the 13th for eleven hours: they were accompanied by non-bilious vomiting, and on the evening of the last-mentioned day they ceased suddenly, and were succeeded by slight shiverings and well-marked jaundice. The bowels having been confined for some days,

a purgative was administered: the fœcal matters were washed, and five calculi with polished facettes were discovered.

Before alluding to the treatment, M. Trousseau entered somewhat minutely into a consideration of the symptoms in these and similar cases. He laid it down as a general rule, that if, in addition to pains of the character described above, there be vomiting of matters not colored with bile, the symptoms depend upon the presence of a calculus in the common duct; and that, ninety-nine times out of a hundred, the presence of bile in the urine will next day confirm the diagnosis. It must, however, be borne in mind that, although the absence of bile in the matters vomited, and the subsequent occourrence of jaundice, entitle us to give a positive diagnosis, the opposite circumstances—namely, the presence of bile in the vomited matters, and the absence of jaundice—do not justify us in absolutely denying the existence of hepatic colic. In most cases the jaundice is so slight as to escape the attention of the patient; indeed, generally it is only by the condition of the urine that its existence is revealed. In addition to pain, vomiting, and the presence of bile in the urine, another diagnostic means is at our disposal. This consists in an examination of the fœcal matters: for this purpose, the evacuations are to be collected from the time of the cessation of the attack during the next three or four days, as a calculus may occupy this time in passing from the duodenum to the rectum, The matters so collected are to be washed in a hair sieve, until the solid matter is completely broken down. Under any other way of proceeding, the calculus might escape detection. It does not always happen, however, that the calculus, the cause of the suffering, is discharged. It not unfrequently happens that there are concretions in the gall-bladder which are too large to pass along the cystic duct. In such cases, the bodies get partially impacted in the duct, and give rise to much suffering; but on their going back into the gall-bladder, the relief is as complete as when they drop into the duodenum. In these circumstances, we are to be guided in our diagnosis by the sudden appearance and disappearance of the symtoms, taken in connection with the absence of symptoms pointing to disease of other organs.

With regard to treatment, nothing can be done to get rid of calculi already existing; our attention must, therefore, be limited to the treatment of the attack of hepatic colic, and to the preventing the formation of fresh calculi. The treatment of the attack is not very satisfactory, and often the remedies employed do more harm than good. Thus many practitioners prescribe opium; but as opium checks all the secretions, with the exception of the secretion of the skin, it is unfavorable to the accumulation of bile in the gall-bladder, which acts beneficially in helping the expulsion of the calculus. The pain, it is true, is deadened by opium, but the expulsion of the calculus is delayed. The relief of suffering is, according to M. Trousseau, more advageously obtained by chloroform or sulphuric ether given internally, while frictions with an alcoholic extract of belladonna, reduced by water to the consistence of a syrup, are made over the seat of pain. The ether is generally given in the form of capsules, each of which contains eighteen drops; in this way a considerable anæsthetic action is produced, while the biliary secretion is not interfered with. Prolonged hot baths sometimes give relief, though in general they produce little effect. When the attack is past, M. Trousseau generally gives, with a view to improving the digestion, seven or eight pills daily, each containing three grains of extract of ox-gall.

The real treatment of these cases, however, is prophylactic. As above stated, M. Trousseau thinks we cannot act upon calculi already formed, although M. Barth supposes that, by a vegetable diet, combined with the use of alkalies and turpentine, biliary concretions may be disintegrated. All we can hope for, therefore, is to prevent the enlargement of existing calculi and the formation of new ones; and this we must do by impending the production of cholesterine. Now, as cholesterine is a fatty substance, we must, in order to diminish its quantity, diminish all the fatty matters of the economy. For this purpose we must look to alkaline substances, which saponify the fats and render them more soluble; to exercise, which promotes their combustion; and to an alimentation which shall contribute as little as possible to their development. Peyrilhe, the first professor of

Therapeutics in the Faculty of Paris, noticed that, in large herbivorous animals, such as oxen and cows, which are very subject to hepatic gravel, the gall-bladder was filled with calculi from the month of April to the month of June, and that it ceased to contain any from the beginning of September until the end of December. He thought that the production of cholesterine was favored in these animals by a dry alimentation, consisting in part of oleaginous grains; while, on the contrary, the chlorophyl, or green coloring matter of plants, acted as a special solvent of this fatty matter. Peyrilhe was right; but he had not noticed another important element in their cases—the influence of prolonged repose. In man, a sedentary life acts in the same way as stalling of oxen; and this is the reason why women are more subject to biliary calculi than men. Green vegetables, though acid, make the urine alkaline; they possess the property of saponifying fats; and you have thus, in exercise on the one hand, and in the use of green vegerables or alkalies on the other, the principal elements of the prophylactic treatment of biliary calculi.

To patients suffering from this affection, we should, therefore, continues M. Trouseau, prescribe walking exercise, a regimen having for its basis green vegetables, to which may be added lean meat and fresh fruit thoroughly ripe. The patient should abstain from eating oleaginous matters, such as fat of meat, butter, oil, milk; he should also partake sparingly of amylaceous or gelatinous matters, which, on account of the large proportion of carbon they contain, have the greatest analogy with fat. We may at the same time recommend the use of alkaline mineral waters, such as those of Vichy, the Mont Dore, etc. Not that these are to be given so as to saturate the system with the mineral principles of the waters, but only in such a way as to improve the general health. They should not be given continuously for too long at a time, but their use should be interrupted and again resumed.

Surgery.

[From the Medical Gazette.]

THE RELATIVE VALUE OF THE DIFFERENT METHODS OF AMPUTATION OF THE LEG AS REGARDS MORTALITY AND THE SUBSEQUENT UTILITY OF THE LIMB.

(Translated from the German, by Alfred Weiller.)

Dr. Boeckel, of Strasbourg, has taken this question into consideration, having published a pamphlet entitled "Les Avantages et les Inconvénients de l'Amputation de la Jambe au lieu d'élection comparée aux Amputation sus Malléolaire et particlles du Pied."

He states that all the statistics lately collected by different authors go to prove the superiority of the supra-malleolar amputation (from the commencement of the lower third to the malleoli) over that immediately, or three or four inches, below the knee-joint; showing that the mortality of the latter is double that of the former. (Fenwick says the relative proportions are 1 in 4.5 and 1 in 2.5.) This decided difference in favor of the first method Dr. B. attributes—

(1.) To the wound being further away from the body.

(2.) To the surface of the wound being smaller; and

(3.) To the comparatively rare occurrence of pyæmia and phlebitis.

As regards the utility attainable subsequent to these operations, Dr. B. thinks the advantages are decidedly on the side of the supra-malleolar amputation, as there is a far better support for artificial contrivances. It is true that some patients, after the latter operation, have to leave off their artificial foot and resort to a wooden leg, in consequence of ulceration of the stump; but in the majority of cases, Dr. B. attributes this to the badly constructed apparatus, in some of which, for instance, the *end of the stump* is made the point of support. And even in those cases where, on account of the tenderness of the stump, and inclination of the same to ulceration, no other apparatus can be used than the wooden leg, the disadvantages and inconveniences

arising from the supra-abundant length of the stump are not so great, but that they are compensated by the difference of mortality, which gives this operation so great an advantage over the higher one.

As regards the operation in the middle third, Dr. B. thinks, without, however, being able to bring any facts to bear upon his opinion, that the large surface of the wound is just as dangerous as in the higher amputation. And, as the stump is not more adapted to an artificial foot, and less so to a wooden leg, he prefers the operation in the upper third to this.

In tibio-tarsal exarticulations, according to good statistical authorities, the average mortality is 1 in 7.2 operations. Out of thirty-two cases cited by Dr. B., four died consequent upon the operation, gangrene of the flaps ensued in three, and in four the caries returned. Out of the two last categories, five cases went through secondary amputation of the leg in loco electionis; ten patients could, after cicatrization of the flaps, use their stumps to more or less advantage; ten could, after some time, walk on the stump without any inconvenience. Dr. B., consequently, comes to the conclusion that this operation is far preferable to the amputation of the leg, provided there is no particular danger of the return of the caries; although, in the former operation, cicatrization goes on slower, and necrosis and suppuration of the sheaths of the tendons take place oftener than in the latter. He also prefers the planta-flap, of Syme, to the dorsal, of Bouden.

The exarticulation below the astragalus, and Pirogoff's operation, are not sufficiently numerous as to allow of a definite conclusion. Dr. B. speaks of seven cases of the first, all of which terminated favorably; and of nine of the latter, one of which was fatal.

In Schopart's operation, the average fatality is one in nine. As regards the usefulness of the limb after this operation, out of thirty-three cases, seven could walk tolerably well immediately after the healing of the wound; in seven, the same result was attained at a later period. In most of the remaining nineteen cases, organic disease of the joint set in about the period

of cicatrization, (in some later); *in all*, retroversion of the heel took place, which interfered more or less with locomotion; in five, secondary amputation was performed, in consequence of returned caries. From further observation of these nineteen and and other similar cases, Dr. B. infers that the retroversion of the heel *depends least upon the contraction of the gastrocnemius*, as in most cases the division of that muscle failed to remove the difficulty; and only in part does it arise from inflammation of the tibio-tarsal joint and the deep-seated muscles of the leg at the period of cicatrization. The chief cause, however, is, in his opinion, the action of the weight of the body upon the astragalus, which is no longer supported by the anterior part of the foot, and consequently, in walking, it is dislocated forward. The highest degree of retroversion he attributes to the relaxation of the articular ligaments. This opinion has been confirmed by experiments upon the subject. His advice is, therefore, not to choose this method in cases of organic articular disease; when, however, this mode is used, the astragalus should be kept, during the period of cicatrization, in a strongly-flexed position; the patient should not be allowed to walk for some time, and then only with a boot the sole of which is considerably heightened in front.

The amputation at the metatarsal bones, and Lisfranc's tarso-metarsal exarticulation, produce better results than all other partial amputations of the foot.

The exarticulation between the os naviculare and os cuneiformis cannot now be passed upon.

Urea.—MM. Poiseuille and Gobley have published a paper full of interesting researches, tending to establish that urea is produced and transformed in various parts of the body, instead of in the kidneys alone; that it is not a substance simply excrementitious, but that, normally, it is taken up in part in the kidneys by the circulation.—*Gazette Hebdomadaire.*

[From the Lancet.]

THE CURE OF VARICOSE VEINS.

A MAN aged about thirty-five years was recently admitted into University College Hospital, with a varicose condition of the veins of the left leg and thigh, associated with a varicose ulcer. From the latter a good deal of blood had been lost lately, and much inconvenience was felt from the presence of several knots and clusters of tortuous veins extending up the inner side of the thigh. The dilated vessels running up the leg to the middle of the thigh were of considerable size. Two pins were placed beneath the veins above and three below the knee, and the usual sutures applied over a small piece of bougie, on the 19th of October; this was done without chloroform being given. The varicosed knots were reserved till a future occasion for injection with the solution of the perchloride of iron. A week later the veins were found to be obliterated, and the effect of this was to diminish the venous knots already referred to. The ulcer had completely healed. Finally, some of the dilated veins were injected with the solution, but not the others, as coagulation had taken place within them. This was followed by some inflammation and an abscess, with a little constitutional disturbance, but the man has gone on well.

We saw some remarkably large varices at the above hospital on the 16th ult., situated at the inner side of the left thigh of a man of sixty, who had a varicose ulcer of both legs, in which the veins were in a varicosed condition. Three pins above and three below the knee of each leg were placed in the usual manner under the veins, and complete obliteration has ensued, with a cure of both ulcers. A second case of varicose veins and ulcers of both legs was submitted to the same treatment, in a man whose constitution was broken down; two pins below each knee, and one above the left knee, were introduced. It is important to observe that this patient had been similarly operated upon three years ago, on his left leg, with a cure of the varix at that time; but the present knot of enlarged veins was an entirely fresh batch. There can be no doubt that in the majority of cases the veins operated upon are completely obliterated; but

we believe, with Dr. Pirrie of Aberdeen, that a permanent cure of the tendency to varix is not accomplished, and that in many cases, sooner or later, the varix returns in some other veins.

The obliteration of varicose veins we saw also attempted at King's College Hospital, on the 22d October, by Mr. Fergusson, the subject being a young woman, a housemaid, who was much upon her feet, and the veins of whose left leg were enlarged and tortuous. A pin was passed underneath a large vessel at right angles to its trunk, and another was introduced through the vein itself, in the direction of its length, so that some amount of inflammation might be induced; a twisted thread was now passed round the pins, with the effect of producing obliteration by the compression thus established.

A REVOLUTION IN ANÆSTHETICS.

The Lancet says that the medical papers of Paris are full of the new method of producing anæsthesia introduced by M. Azam, of Bordeaux.

It would appear that, about eighteen months ago, M. Azam had under his care an hysterical young lady, who was subject to cataleptic attacks. Upon this patient very extraordinary phenomena were noticed, which, coming to the ears of M. Bazin, professor at the Faculty of Sciences of Bordeaux, this gentleman advised M. Azam to consult a work published in England in 1842 by Mr. Braid, in which the means of producing catalepsy and artificial anæsthesia were detailed. M. Azam procured this book, of which Dr. Carpenter has written an analysis in Dr. Todd's Cyclopœdia, under the head of "Sleep," and began a series of experiments on his patient and about thirty other persons. He found that most of Mr. Braid's statements were correct, and that catalepsy and anæsthesia could actually be obtained in the following manner:—

The patient, either sitting up or lying down, is put in a convenient position. The operator then, standing either before or

behind him, places before his eyes, at the distance of a few inches, but generally nearer than the point which allows of distinct vision, some bright object, upon which the patient should steadily and continously fix his eyes. The bright object should be so placed that the eyes, in looking at it, must be forcibly directed upwards, the contraction of the superior recti being carried to its maximum degree. In this position, the levatores palpebrarum and recti are strongly contracted, and convergent strabismus takes place. After this attitude, which is certainly fatiguing, has been kept up for two or three minutes, the pupils are noticed to contract, and soon afterwards to dilate; the eyelids quiver rapidly, then fall, and the patient is asleep. Two symptoms, almost always present, are then observed; they are, however, in different cases, more or less marked and lasting: 1, catalepsy, exactly as described in books; 2, anæsthesia, which lasts from three to fifteen minutes, either complete or incomplete, but which allows of pinching, pricking, and tickling, without any feeling being aroused in the patient, and without any change in the cataleptic state being produced. This anæsthetic state is generally followed by a very opposite condition—namely, very remarkably hyperæsthesia, in which the senses, the feeling of heat, and muscular activity reach an unusual degree of excitability. At any moment of the experiment the symptoms may suddenly be stopped, by rubbing the eyelids, and directing upon them a stream of cold air. When the patients recover their senses, they remember nothing of what has taken place.

Several experiments have been instituted in Paris by Messrs. Folin, Broca, and others; and M. Velpeau seems so convinced, that he has presented a short paper on the subject, by M. Broca, to the Academy of Sciences at the meeting of the 5th instant.

Ozena.—Dr. St. Arnould recommends the following injection to be thrown in or inspired night and morning, after having cleaned out the nares with some injections of tepid water: chlorate of potass, 32 parts, boiling water, 1000 parts.—*Presse Belge.*

Midwifery.

ON THE CICATRIX-LIKE STREAKS ON THE SKIN OF THE ABDOMEN, BREASTS AND THIGHS, OF WOMEN THAT ARE PREGNANT, OR HAVE CHILDREN.

Opinions as to the time of appearance, frequency, and other peculiarities, and medico-legal importance of these streaks, are so unsettled and conflicting, that we shall gladly hail the excellent review of the subject by Credé, of Berlin, one of the editors of the "*Monatsschrift für Geburtskunde und Frauenkrankheiten.*" We have prepared for our readers the following propositions which he has established: (Monatschr., &c., Nov. '59, p. 323 *et seq.*

1. The streaks on the abdomen more or less extensively exist in the great majority of pregnant females. They appear but very seldom, however, during the first half of pregnancy—frequently not until the last month, or the last but one.

2. Soon after delivery they change in appearance, becoming gradually less evident, unless the skin is made tense, but never entirely disappear.

3. In some cases they do not appear during pregnancy, and sometimes no trace of them can be found after repeated pregnancy, and sometimes no trace of them can be found after repeated pregnancies.

4. Sometimes they appear for the first time at the second or third pregnancy, or else new streaks are added to the old.

5. They may make their appearance, also, without the existence of pregnancy—in consequence of diseases producing a rapid and considerable extension of the skin, (especially in dropsy, therefore).

6. The quite similar streaks on the breasts, and the anterior surface of the thighs, occasionally, also, on other parts of the body, as the buttocks, calves of the legs, etc., merit the same attention as those on the abdomen.

TO EXCITE PREMATURE LABOR.

Dr. Baum objects to many methods the danger of too easily rupturing the membranes. He employs cat-gut bougies of a foot long and two or three lines thick; he steeps the end in soft water to full softening, and passes it, well-oiled, into the uterus, by means of twisting motions, until a length of only one or two fingers' breadth remains in the vagina. Uterine contractions are set up in from six to twenty hours. The bougie never (?) injures the membranes; it may be removed shortly before the rupture of the membranes or the birth of the child. During 1857 or 1858 the author had twelve opportunities of employing this uterine catheterization. By its means eleven children were born alive, five dead; eight mothers recovered, and no one sank from any puerperal process. The modes of death were — one from pneumonia, one from miliary tuberculosis, and two from Bright's disease, In estimating the risk of rupturing the membranes by this method, it is right to bear in mind the author's precautions in using very flexible catgut-bougies, and softening them for use.—*Monatschr. für Geburtsk.*

SEX DETERMINED BY THE MOVEMENTS OF THE FŒTAL HEART.

Doctor Frankenhauser asserts (in the Monctsschrift für Geburts-K) that whilst paying attention to the comparative frequency of the beats of the fœtal heart, before and during labor, he noticed that the heart beat with less rapidity in the male fœtus than in the female. The pulse in the former is on the average 124, in the latter 144. Upon these data the author has almost invariably predicted the sex.

If the assertion is true, the correct explanation of the fact will be very difficult: it is known that as a general rule the pulse of girls is more rapid than that of boys of the same age.

TREATMENT OF UTERINE HEMORRHAGE BY LARGE DOSES OF ERGOT AND DIGITALIS.

By numerous experiments with regard to uterine hemorrhages, Dr. Trousseau came to the following results: the ergot of rye is the safest and promptest remedy in post-partum hemorrhages, and in those following after abortion. In hemorrhages at the change of life, it is slow and uncertain in its action, while ratany and sulphuric acid are more reliable. In bleedings from carcinoma its effect is remarkably prompt. Dr. Trousseau does not believe in its specific styptic effects, but is inclined to attribute its anti-hemorrhagic qualities only to its action upon the muscles of the uterus. The digitalis was applied by Trousseau in doses of from 0.50 to 1 grmm. pro die in one case only, which left the question about its efficiency undecided. The remedy is principally advocated by Mr. Howship Dickinson.—*L' Union*, 36.

New Anesthetic.—Dr. William H. Uhler, of Falls of Schuylkill, at a recent meeting of the Academy of Natural Sciences, mentioned that he had, a short time before, accidentally inhaled the vapor of the bisulphide of carbon, which had produced complete anesthesia. He was removed from the laboratory by the workmen in a completely insensible condition. He revived in a short time suddenly and completely, and he did not subsequently experience any nausea, or the least unpleasant symptom. Whilst in a state of anesthesia, his visions were of the most pleasant and agreeable character.—*Phil. Med. News.*

Nitric Acid has been strongly recommended in bronchial catarrh, as also in whooping cough; in the latter affection, according to Arnoldi and others, the acid is as positively curative as quina is in the miasmatic disease.

Editorials and Miscellaneous.

THE NEW YEAR.

Another year has been "numbered with the things that were." It died in child-bed, on the 31st of December last, as many a predecessor before it; and though it had festooned the Earth with flowers, gladdened the fields with a grateful harvest, decked the forests with the rich sheen of the autumnal foliage, and dispensed the choicest blessings with regal prodigality, there was not a single tear shed upon its grave. It had grown old, it is true,—but does Time weaken the bonds of friendship? It had become useless,—but does affection weary because the ability to confer benefits is lost? It was to bring forth an heir — the representative of its power, and the inheritor of its glory,— but do men turn from the setting to the rising sun, when in the mood for reverence and worship?

Let others answer these questions according to their experience, we merely wish to mention the fact, that in this instance, as well as in every other for centuries past, the *old* year was forgotten, whilst the fire was piled higher, and the goblet was drained to the bottom, in honor of the *new*.

There is wisdom and mercy in all things if they be interpreted aright—for so revelation declares and experience demonstrates; and though, as finite and fallible creatures, we are disposed to forget the past and regard only the present or the future, there may be embodied in this very infirmity of human nature, a wise provision for the happiness and improvement of the race. The truth is, there is such amount of actual and unavoidable sorrow connected with this life—there are so many natural and inevitable cares associated with man's destiny in this existence, that there must be a vast deal of true philosophy in looking upon the bright side of the picture under all combinations of circumstances,—and even in worshipping the "rising sun" as it "comes rejoicing in the East," rather the waning luminary of the western horizon with all its grand but fading glories.

But we do not wish to moralize. Our principal object in referring to this subject, is to tender to the patrons of the Journal our kindest acknowledgments for their past liberality, and to wish them, one and all, a happy, healthful and delightful New Year! May their shadows never grow less, and their pockets continue in a state of "redundancy and plethora!" May they be blessed with every good thing, from an abundance of practice to punctual patrons, inclusive! And may they never forget the cardinal principles of our noble brotherhood,—those characteristic of the real gentleman,—those ornaments of the sincere Christian,—those attributes of the good Physician—charity, fidelity and truth!

The past year has been an eventful one, and will stand forth in bold relief upon the world's history. It has brought forth both good and evil things abundantly,—but so commingled and combined,—in so strange and heterogeneous a medley,—in such wild confusion and contradiction, that nothing but Divinity itself, or its faithful servant Time, can separate and arrange them. It has produced grand progressions and great retrogressions,—wonderful improvements and extraordinary lapses,—a glorious civilization and a gloomy barbarism. It has witnessed immense social reforms and unexpected political revolutions,—the triumph of the battle-field and the glory of the olive-branch,—the victory of despotism and the freedom of shackled nations,—the apotheosis of money and the elevation of letters,—the abasement of religion and the exaltation of the church. It has been emphatically the era of scepticism and the age of faith,—the epoch of humanity's greatest glory, and the witness of its most abject humiliation,—the proof of man's divinity and the evidence of his corruption. And in our country, it has lighted the torch of fanaticism, and fired up the heart of the patriot; it has strengthened the hand of prejudice and treason, and bound all true men the firmer and more loyally together; it has crimsoned our soil with fraternal blood, and vindicated the stern demands of justice by an expiation upon the gallows; it has shaken our noble institutions, and proved, we fondly hope, to a jealous world, that

their foundations are laid too deep and strong in the affections of the American people, to permit the *possibility* of their destruction.

Such has been the history of the past year; and though we may not be able to bring order out of the chaos,—symmetry from these irregularities, or light from this darkness,—though we cannot discover the nucleus around which the disorganized elements may finally arrange themselves,—though we cannot distinguish the precise point to which we are drifting, both socially and politically,—we must abide the result with patient resignation, remembering that our destinies are in the hands of Him who ordereth all things well, and whose noblest attributes are justice and mercy.

It must, however, be a source of pride to every man in our profession to know, that in despite of skepticism and envy,—in the very face of the most powerful and uncompromising opposition that ever arrayed itself against any human system,—in defiance of open foes and sneaking traitors,—the career of Medicine has been upward and onward, and that the glorious fact is generally appreciated. For, disguise the truth as our adversaries may, the history of progress finds no grander or more beautiful illustration, than that exhibited by the various departments of which our exalted science is composed. We are aware that it has become fashionable in certain quarters to deny the truth of the fundamental propositions upon which the whole superstructure is reared, to question its title to the rank and dignity of a science, and to degrade it to the subordinate position of a mere conjectural art. Taking advantage of the skeptical proclivities of the age, the enemies of our noble calling have sought to propagate a spirit, not of honest inquiry,—not of candid investigation,—not of faithful research,—but of doubt, distrust and suspicion in regard to the grounds upon which Medicine claims the respect and confidence of men. Anxious for some ephemeral notoriety, or imbued with that restless and polemical spirit which distinguishes the literary adventurers of the present era, they have sought to sap the foundations of our faith, and to demonstrate to the world that

our position is weak and untenable. And we regret to say it, but it is unfortunately and disgracefully true, that some of the most cunning, astute, and malignant of our enemies, belong to our own household,—are sailing under our time-honored flag,—are, or profess to be, legitimate sons of Æsculapius.

Had we space, it would delight us to enter into a systematic defense of Medicine,—to show the stability of those great principles upon which it is reared,—to prove that it is a veritable science in the fullest and most emphatic meaning of that term. Unfortunately, however, this is impossible under existing circumstances, and we shall content ourselves with knowing that wherever the light of civilization has penetrated,—wherever the chains of ignorance have been rent from the human mind by the magic touch of knowledge,—wherever Religion has proclaimed the "glad tidings of salvation"—from every enlightened nation under the broad canopy of Heaven,—an anthem of praise and thanksgiving is continually ascending to the skies for the countless blessings resulting from the benignant ministrations of our most exalted calling.

OUR JOURNAL.

In order to get a fair start, and to enable our publisher to complete his arrangements for a permanent location in Edenton, we have delayed the publication of the Journal until the present month, making thereby a slight but permanent alteration in the time of its appearance for the future. The next number will be issued on the first of March, and the subsequent numbers on each alternate month, from that date, until the volume is completed.

We had hoped that the Journal would be issued early enough in January for the "flood tide" of the hilarious feelings inspired by the season, so that it might find our patrons in the humor to overlook its defects, and to *remember the Editor;* but the desertion of hands at an unexpected moment, so paralized our publisher, that he was necessitated to delay it to the present

moment. Our friends must not suppose that its prospects are at an "ebb," or that there has been any "abatement" of energy on our part. It was never in a more prosperous condition in every particular, and our interest in its success has increased instead of diminished. The next number *shall be published early in March*, at all hazards. In the meantime, we must ask every friend of the Journal to show his interest in the enterprize, by inducing others to subscribe.

DR. IGNATIUS LANGER.

It is rank treason to speak one single word against the National Medical Association. That body is presumed to be immaculate, and the slightest whisper of condemnation carries with it a conviction of corruption on the part of him who dares its utterance. It is regarded as something sacred,—especially by those who expect its favors,—and the man who disputes the fact is immediately deemed guilty of sacrilege. It is invested with such an immense amount of dignity and importance, by certain interested parties, that any exhibition of distrust, disregard, or dissatisfaction, from whatever quarter it may come, is visited with as much "indignation and wrath" as if it were *the* unpardonable sin. For these, or other reasons, it has become fashionable not only to cover all of its acts with the most lavish encomiums, but to make of every one who has the temerity to indulge in the least independence in this particular, a target at which to discharge every possible missile of the intellectual armory. Now, as there was born in us an uncompromising hostility to all that the term *fashion* defines, and as we believe that every man should think and act for himself, without regard to opinions or consequences, we cannot refrain even from arraigning this "august body."

That it has accomplished much good we freely confess. That some of its "Reports and Essays" reflect credit upon its members and honor upon the country, no one can deny. That it

has promulgated a wise and salutary ethical code for the government of the profession, and has taught some wholesome lessons to evil doers, are historical facts. That it has developed a community of interest and sentiment among the members of our calling, and, by uniting the physicians of the land into a common brotherhood, has strengthened the bonds which bind us together as a nation, must be acknowledged by all disinterested men. But that it has made sad mistakes in regard to matters of the first importance,—that it has committed grave and serious errors in many things,—that it has been managed unwisely, selfishly and indiscreetly,—and that it is becoming a hot-bed for the development of mediocrity, a nursery for the cultivation of personal vanity, and a grand advertising medium for professional hucksters and charlatans,—can be established by appealing to the record itself.

If details were necessary in this connection, they might be furnished without difficulty, but a few familiar facts will suffice for the present occasion. Is it not the boast of the Schools that the National Association is impotent in regard to the great subject of medical education? Has it not been repeatedly defied by the enemies of such reforms as the best interests of the profession imperatively demand? Has it not been constrained by outside pressure to ignore and repudiate the "diploma qualification"? Has it not admitted "self-constituted" delegates, and fraternized with men who had been expelled from State and local societies? Has it not honored those who had been convicted by its own code of unprofessional conduct? Has it not received into its bosom the vendors of patent medicines, the hawkers of artificial legs, and a whole multitude of quacks? Have not men been appointed to report on the most important subjects, who were utterly unacquainted with them — and that, too, at their own solicitation, or through the management of friends? In a word, have not the proceedings of this great body been conducted with so much haste and carelessnes as to admit of management on the part of the adroit, and to bring dishonor and ridicule upon the profession of whose views it is the authorized exponent? There

can be but one answer given by those who are acquainted with the subject, and, we regret to say, it is an affirmative one.

We have been led into these remarks by the case of the individual whose name is at the head of this paper; and in order to insure their appreciation by our readers, we will give a short history of it: In Louisville, last year, we observed a rough-looking foreigner, who we mistook for one of the employées of the Association, as he seemed decidedly out of his element among its members, until we found him finally mounted upon the stage, reading from a manuscript, which he had some difficulty in deciphering, and with so decided a Teutonic accent as to be generally unintelligible. No one seemed to be acquainted with him,—but all were amused by the exaggeration of his manner, the peculiarity of his appearance, and the grotesqueness of the whole picture. He had no sooner taken his seat, however, than a motion was made, that he be appointed a special committee to report on the subject of his essay, and Dr. Ignatius Langer went forth to the world *endorsed* by the National Medical Association of the United States. From a circular issued by the Medical Society of Scott county, Iowa, and published in the principal journals of the West, it appears that this man was a "self-constituted member" of the Medical Association, and that he has been formally expelled from his County Society for low and unprofessional practices.

This case should, and must teach an important lesson even to those who believe in the infallibility of the Association; for if it is thus to be prostituted and humiliated before the world,—if such shameless obliviousness of the claims of self-respect, and the interests of Medicine is to be associated with its history,—if it is to be made the grand reservoir of such putrescent materials as we have just ventilated,—then are the days of its existence numbered, and *sic transit gloria mundi* is its inevitable epitaph.

CONSERVATISM.

We cannot refrain from expressing the gratification we have experienced at the conservative position which many of our northern cotemporaries have assumed in the present political crisis. They speak like bold and true men in defense of the rights of the South, in support of the Constitution and the Union, and in opposition to that wild spirit of fanaticism which would apply its impious torch to the noblest institutions that God ever crowned with his blessing. They assure us of their sympathy, of their unwavering friendship, and of their determination to stifle the serpent whose unholy presence has disturbed the harmony of our political Paradise. They appeal to us as brothers, as philanthropists, and as patriots, to keep our beloved profession "pure and unspotted" from the world of strife and turmoil, by which the criminal infatuation of factious men has surrounded it in these degenerate days. And they assert with enthusiastic fervor, that the glorious "flag of stars and stripes" *shall* never be rent in twain, but that to our profession shall pertain the distinguished honor of proving the strongest and most enduring band which binds this magnificent constellation of States together.

We believe the wish is father to the thought, and we pray to Heaven that their predictions may prove propheeies in the end, —for repeated aggression has excited the southern mind almost beyond endurance, and fanaticism may teach it that forbearance is no longer a virtue, but a crime against nature itself— and one of which the veriest coward beneath our sunny skies will be ashamed. Every instinct of patriotism, every sentiment of humanity, every principle of religion, should constrain us, then, to stand by those patriots of the North, who, without regarding their position, considering their personal interests, or consulting their early prejudices, are nobly battling for the preservation of the constitution and the perpetuity of the Union,—and who beseech us by all that is proud, and elevating, and endearing in our noble profession, to preserve it from geographical lines and sectional divisions.

We do not speak as a partizan — for we know no politics in this connection, — but from an ardent love for our country, and a devotion to our exalted calling which no language can express, or lapse of time destroy. And whatever may be the consequences, we cannot refrain from expressing the hope that a brighter and better day is yet to dawn upon our distracted land, and that this Union — like the rock which crowns some stately mountain peak, now tottering to the gentle breath of the zephyr, but anon mocking the wild fury of the storm, — may still loom up amid all coming ages, to guide the exile of every clime to a haven of rest, and a home of happiness.

NEW JOURNALS.

Our thanks are due to Drs. Davis & Steele, Editors of the Chicago Medical Examiner; to Drs. Heddens, Chambers & Scearce, Editors of the St. Joseph's Journal of Medicine and Surgery, and to Dr. Baker, Editor of the Cincinnati Medical and Surgical News, for copies of their publications.

These are all new Journals, well conducted, and really deserving the patronage of the public. We wish them all the honors, with none of the troubles of the life Editorial. It is really astonishing to observe the number of Journals constantly springing up in the country, and if the demand is equal to the supply, the literary character of the profession must be improving decidedly, — the perverseness of the Schools to the contrary notwithstanding. We had the pleasure of meeting Dr. Davis in Louisville, where he went to talk with his "*children*" something about *milk*, we believe — though he did fail most signally in *bringing them to it*, we must confess. He is a most excellent gentleman, though, and the Association need not be ashamed of its paternity.

The Maryland and Virginia Medical Journal. — Our old friend, the Virginia Journal, is hereafter to appear under this name, with Dr. Van Bibber, of Baltimore, as one of its editors. We wish it all possible success.

The Ohio Medical and Surgical Journal.—This excellent bi-monthly is under the especial charge of our friend, Doctor J. W. Hamilton, of Starling Medical College,—a gentleman who impressed us as favorably in every particular, as any member of the National Association. The Journal is conducted with marked ability, and in a spirit of independence which does honor to the Editorial fraternity. It affords us much pleasure to reciprocate the courtesy of an "exchange."

Dr. David Cox.—By a typographical error a mistake has been made in the name of the gentleman whose excellent Report is published in this number. We tender him our apology for the blunder, and thank him kindly for his sensible and practical communication.

ITEMS.

Dr. Rogers, Dean of the Faculty of the University of Pennsylvania, announces, officially, that only 18 Southern Students have left *that* Institution, whilst the Philadelphia papers declare that the whole number of seceders was less than 200. In New York a meeting was held to consider the propriety of leaving, but as only 14 agreed to do so, the whole thing was abandoned.

Dr. Thomas Smethurst, who was convicted of wife-murder, in England, some months since, has been pardoned by the Crown, on the ground that the medical evidence was insufficient, such being the opinion of Sir Benjamin Brodie.

There were 26003 births registered in Scotland during the last year—of which 23,699 were legitimate, and 2304 illegitimate,—which shows that the land of John Knox and the Kirk can do "some things as well as others."

Drs. Wilson, Professor of Technology in the University of Edinburg, and Laurie, Professor of Surgery in the University of Glasgow, are dead,—both able and honorable men.

Curare is being used in Europe for the cure of Tetanus, and has elicited a warm debate in the French Academy. Velpeau believes it to be a dangerous and useless remedy, whilst Bernard and Chassaignac have considerable faith in it.

The Physicians and Surgeons of Guy's Hospital have awarded the Ashley Cooper Prize, of $1500, to Dr. Crisp, of London, for his Essay on "The Structure and Use of the Thyroid Gland."

Suicide is on the increase in Northern Europe, says the Nashville Journal. In France there are 10 in every 100,000; in Belgium, 5; in Russia, Sweden and Norway, 11; and in Denmark, 26.

Gutta Percha.—A new tree, producing this material, has just been discovered at Suriam.

The French nation has just erected a statue to Jenner, in front of the Louvre.

Professor Agassiz has returned from Europe with renewed health, and important materials for his museum of Natural History.

M. Chassaignac recently punctured a carotid aneurism, mistaking it for an abscess, and was then compelled to litigate the common carotid, which he did with success. The patient has recovered, with no bad symptoms.

Dr. S.W. Butler, senior editor of the Medical and Surgical Reporter, has been appointed Chief Resident Physician at the Philadelphia Lunatic Asylum.

Prize Essays of the American Medical Association.—All essays offered for the prize must be sent, on or before April 1, to some one of the Committee, who are—Drs. Worthington Hooker (Chairman), New Haven, Conn.; G. C. Shattuck, Boston, Mass.: Usher Parsons, Providence, R. I.; P. A. Jewett, New Haven, Conn.; and Jonathan Knight, New Haven, Conn.

Mr. S. T. Trowbridge, of Decatur, Ill., has invented a physician's cane. It consists of a hollow tube, closed at its bottom, and having a semi-tube attached to the knob or handle, and fitted within the cane, and allowed to move freely in and out of it, and forming a receptacle for vials containing medicines.

It is stated that 4,000,000,000 pounds of tobacco are produced annually, and that $400,000,000,000 are expended for it by the human family.

Therapeutical Record.

Treatment of Scarlatinous Angina by Iodine.—

℞.—Iodide of potassium	1	grain.
Iodine	0.1	"
Chlorate of potassa	4	grains.
Nitrate of "	6	"
Aqueous solution of potash	4	"
Water	240	"

Administer one or two teaspoonsful every four hours, according to age of the patient.—*Gazette Médicale de Lyon.*

Varicose Veins.—Before the King's County Medical Society, as reported and published in the November number of the New York Journal of Medicine, Dr. Enos reported a case of varicose veins successfully treated by injecting *persulphate of iron.* He is of opinion that this is the first time persulphate of iron has been used for this operation, and he thinks it will prove a better preparation, because of its less irritation than the perchloride.

Nocturnal Sweats.—M. Delioux, chief of the French naval surgeons, after numerous experiments with the different preparations of bark in the treatment of colliquative sweats, which occur during sleep in phthisis and other diseases, considers the tannate of quina, introduced by Barreswil, the most appropriate remedy in this affection. This agent is supposed to be peculiarly applicable to two essential conditions in this symptom—organic debility and periodicity. M. Delioux cites many cases in support of his views. He administers this agent in two or three doses of six or eight grains during the afternoon. It is insipid, and causes neither wakefulness nor indigestion.—*L' Union Médicale*, No. 43.

An Antiseptic Mixture for the preservation of blood globules, nerves, ganglions, the retina, and the white tissues generally, is recommended by Dr. Passini, to be made as follows: Protochloride of mercury, 1 part; chloride of sodium, 2; glycerine, 13; distilled water, 113 parts.

Chlorate of Soda.—Chlorate of soda is considerably more soluble than the corresponding potash salt; it may consequently be given in a much smaller quantity of vehicle, and moreover the taste is less disagreeable. It has been employed with uniform success in several cases of diphtheria.

Relation of Color Between the Hair and the Brain.—M. Gubler is of opinion that there is a constant relation between the color of the skin and of the cerebral matter. He says that he has thus been enabled to distinguish the brain of a negro amongst seven or eight others.

Icterus.—According to Kühne, the conversion of benzoic acid into hippuric acid, which invariably occurs when benzoic is taken internally by a healthy individual, does not obtain in cases of jaundice. The benzoic acid is eliminated by the kidneys without having undergone any change.

Rheumatism.—We recommend to our readers the following prescription for the cure of this most unmanageable disease:

℞.—Liq. potassæ.....qtt. xv.
Potass. iodidi.....grs. ij.
Mucil. acaciæ.....℥ j.
Aq. distill.....℥ xi. M. ft. haust.

℞.—Potass. iodidi.....ʒ ij.
Morph. muriat.....ʒ ss.
Ung. cetacci.....℥ iss. Ft. ung.

℞.—Opii purif.....gr. j.
Extr. colch. acet.....grs. ij.

Gonorrhœa.—In the Oglethorpe Medical and Surgical Journal, for November, Dr. M. D. Mooney, of Georgia, reports his experience in the treatment of gonorrhœa with the extract of the Indian Cannabis. He says: "I used the following prescription in four cases of gonorrhœa, and was successful in every case, in from five to seven days:

℞.—Sugar of milk.....℥ ss.
Ext. Indian cannabis.....20 grains.

Mix well together and divide into 60 powders, one to be taken every three or four hours. This prescription, I am persuaded, will relieve the most obstinate cases in a short time."

Chloride of Zinc.—In the London Lancet Dr. G.W. Spence recommends the following formula for a chloride of zinc plaster: Dissolve fifty grains of prepared chalk in two drachms (by measure) of commercial muriatic acid, dissolve one hundred and fifty grains of sulphate of zinc in two fluid drachms of boiling water. When required for use the two solutions are to be mixed, and they will produce a paste weighing nearly an ounce, and containing about a sixth of pure chloride of zinc.

Strabismus.—Du Bois Reymond (Müller's Archiv.) recommends the *steroscope* for the cure of squinting. The principle is the same as in the case of prismatic spectacles advised by Mr. Wells.

Dyspepsia.—*Messrs. Editors*: In the course of a long practice in cases of indigestion, I have found the following prescription to have done good service:—

℞.—Prepared carbonate of iron, calcium of magnesia, pulverized elm bark, each..... ℥ i.
Pulverized cubebs..... ℥ ss.
M.

Take a teaspoonful, half an hour before eating, in half a tea-cupful of water.—*Boston Med. & Surg. Jour.*

Gustative Sensation.—Klaatsch and Stich have reported some investigations upon this subject. Their experiments were made with extract of quassia, solutions of sugar, salt and tartaric acid. These substances were applied by means of a very thin camel's-hair brush, while the eyes of the person operated upon were tied. The authors infer: "A narrow border round the margin of the tongue, varying from two to four lines in breadth, is subservient to taste: in most persons it lies in the middle of the margin; in some it approaches the upper surface; in very few, it appears, the lower surface. The root and the posterior third of the tongue, as well as a part of the soft palate, are also subservient to taste. Other parts of the mouth or fauces, or of other organs, are not instrumental in this sense."—*Virchow's Archives.*

Spermatorrhœa.—Dr. Lucien Corvisart, a pupil of M. Chomel reports three cases of obstinate nocturnal, and even diurnal seminal losses, which were completely cured by the use of digitaline. We refer those who desire to learn the details of the treatment to Dr. Corvisart's paper.—*Bulletin de Thérapéutique.*

Chloroform.—When opium is contra-indicated, or fails to act, in cases where it is desired to procure sleep, give thirty or forty minims of chloroform suspended in a little acacia mixture, or some other mucilaginous liquid. It generally succeeds in procuring for the patient two or three hours of tranquil sleep.—*Braithwaite's Retrospect.*

NORTH CAROLINA

MEDICAL JOURNAL.

MARCH, 1860.

Original Communications and Translations.

A REVIEW OF DR. OTIS FREDERICK MANSON'S ESSAY ON "MALARIAL PNEUMONIA."

BY W. T. HOWARD, M. D., OF WARRENTON, N. C.

[*Presented to the State Medical Society of North Carolina May*, 1858.]

(CONTINUED.)

We will now proceed to offer some observations upon Doctor Manson's views respecting the

"*CAUSE*" OF "MALARIAL PNEUMONIA."

Dr. Manson believes that the efficient cause of this disease is *malaria*, by which he "desires to imply the identical cause of that class of pyrexiæ now generally known as periodical fever." He eschews any discussion of the different theories that have been enunciated, concerning the origin of this class of diseases, preferring to designate that latent and subtle principle, productive of them, by the convenient title of *malaria*, without committing himself in regard to its essential nature.

Under this head Dr. Manson enters into some speculative remarks in relation to the manner in which reduced temperature injuriously impresses the lungs during the cold seasons of the year, and especially when the system has been enervated by the depressing influence of malaria, which, he thinks, possesses an evident inherent tendency to produce internal engorgements. We need not go over this ground with our author, as his views,

in respect to the topic now in hand, may be thus grouped together:

"By some, an explanation of the existence of this disease has been given, by attributing it to the combined agency of malaria and atmospheric vicissitudes. This is, doubtless, often true, but it does not offer a satisfactory solution of the fact that many are often attacked with it, who have not been exposed in any appreciable degree to those changes. Indeed, this affection often attacks a number of persons consecutively in the same household, with intervals of time elapsing between the occurrence of the cases to permit all conditions of temperature and humidity to appear, properly belonging to the season. 'It is probable,' says Lænnec, 'that the epidemic peripneumony is often owing to deleterious miasms which have entered the system by means of the cutaneous or pulmonary absorbents, since nothing is more common than to meet with cases of this disease to which we can assign no occasional cause. How many persons are seized with it in their very chambers, and in spite of the utmost care taken of their health.'* It is impossible that any observer of ordinary perception can have failed to have been often impressed with this reflection. Exposure to atmospheric alternations does not, therefore, offer a complete and satisfactory explanation of the general occurrence of this affection."

After having given his explanation of the modes by which cold and malarial influence may act as exciting and predisposing causes of this affection, Dr. Manson adds:

"But this solution does not wholly apply to those cases occurring in previously healthy subjects, whilst sedulously guarded from atmospheric vicissitudes. These cases are evidently due to the evolution of malaria, the affection of the lungs being produced by the predisposing tendencies of the season. Objection has been urged on many grounds to this view of its causation, the principal one of which is that pneumonia prevails in seasons inimical to the existence of this mysterious agent. This is plainly an error. Primary attacks of periodic fever frequently occur during the winter and spring months, especially in the latter, which is also the season in which this form of pneumonia is usually prevalent. The opponents of the views I have embraced seem to ignore this established fact, as old as the ancient couplet—

'An ague in spring
Is fit for a king.'" †

We do not propose to follow Dr. Manson, to any great extent, in his reflection on this topic of inquiry; but one cannot help remarking that his reasoning, in the premises, does not appear to be either very clear or satisfactory.

* Forbes' translation, 233. † Elliotson's Practice, 242.

He states, in the first place, that "malarial pneumonia" can not be due, in all cases, as he alleges is maintained by some, to the "combined agency of malaria and atmospheric vicissitudes," for the reason "that many are often attacked with it, who have not been exposed, in any appreciable degree, to those changes;" a number of persons being frequently attacked, "consecutively, in the same household"; and yet affirms, in the same breath, that there are "intervals of time, elapsing between the occurrence of the cases, to permit *all conditions of temperature and humidity to appear, properly belonging to the season*"! A most singular reason, certainly, to be assigned, why the disease in question cannot then be connected with the action of "atmospheric vicissitudes." These cases, he subsequently tells us, are met with in "previously healthy subjects, whilst sedulously guarded from atmospheric vicissitudes," and "are evidently due to the *evolution* of malaria, the affection of the lungs being produced by the "predisposing *tendencies of the season.*" But are malarial emanations — those that give rise to the prevalence of periodic fevers—"*evolved*" during winter, and the cold seasons of the year? And what are the "predisposing *tendencies of the season*," unless they be connected with those "seasons of *low temperature*," during which febrile affections are so prone to be complicated by "lesions of the lung parenchyma," the *modus operandi* of which, in producing the latter, he endeavors to explain? How, then, can he charge the opponents of his views, respecting the causation of the cases under notice, with "obvious inconsistency" in attributing their occurrence to the "combined agency of malaria and atmospheric vicissitudes," when they are encountered in those seasons of the year that are considered by them destructive of malarial influence; and yet aver, himself, at the same time, that "these cases are evidently due to the *evolution* of malaria," aided by the "predisposing *tendencies of the season*"? The "*inconsistency*," of which Doctor Manson speaks, lies, obviously, with himself,—unless, forsooth, he is prepared to maintain that miasmatic exhalations do not cease on the advent of cold weather, which will not be done, of course, by one so enlightened, and who has made the subject

an object of special study; for, as Doctor La Roche remarks, "every one, from the learned to the gossipping granny, knows full well that the accession of severe cold weather, and particularly of black frost, is sure to be marked by a cessation of malarial fevers." If primary attacks of these fevers, as well as instances of "malarial pneumonia," are met with, as Doctor Manson correctly states they may be, during the winter and spring months, it is in consequence of a well-known law of the poison of malaria, by virtue of which (as Dr. Manson has expressed in the form of an "*axiom*,") "periodical fever may owe its existence to an exposure to its cause *at a period considerably antecedent to its development*," whenever any sufficient exciting cause comes into play, without the occurrence of which no proof may be presented that the system has been impressed by a malarious influence at all. It is, surely, not easy to understand why a particular class of cases cannot be induced by the "*combined* agency of malaria and *atmospheric vicissitudes*," and yet be induced by the "*evolution* of malaria" and the "predisposing *tendencies of the season*," which, according to Dr. Manson, are nothing more than "the usual effects of reduced temperature upon vascular tissues in general," especially potent when the system is contaminated by the depressing influence of malaria. Indeed, it seems to us that Dr. Manson only employs somewhat different phraseology to express the same idea; for, when the several phrases are analyzed, they mean the same thing; still, he rejects the idea conveyed by one of the modes of expression, and adopts that conveyed by the other!

The fact is, when we come to examine, a little closely, into the usually alleged causes of pneumonia, we shall find that they have not been determined, in all cases, with as much certainty as some authorities have been disposed to represent. Exposure to cold, especially when the body is heated and perspiring, is generally assigned as the most prominent exciting cause; but we often meet with cases, which, at all events, cannot be *traced* to that, nor to any other appreciable cause. Great caution is necessary in determining, with precision, the influence of *sudden chill*. Grisolle investigated this subject with his usual care, and

with the most anxious desire to ascertain the truth. He directs attention to the easy sources of error in obtaining accurate information in respect to this influence, and shows that if we ask the direct question — were you suddenly chilled while in a heated state? we shall almost always be answered in the affirmative: for the patient frequently mistakes the increased sensibility to cold, which precedes the onset of most acute diseases, for the rigors of the invasion; and frequently he ascribes the attack to cold, because he deems it the only mode of accounting for his sickness. Of 71 cases, the histories of which were inquired into with the greatest case, Chomel found that only 26 had been *chilled* during the preceding days.* Grisolle found that of 205 patients, only 49 could be distinctly traced to having been suddenly chilled. Here is a table showing the results of his ininquiries on this point, in connection with those of two other accurate observers:

	Cases.	Seizure preceded by sudden chill.
Chomel	79	14
Barth	125	38
Grisolle	205	49
	409	91

It is thus seen that pneumonia could be traced to the influence of *sudden chill* in only 91 out of 409 cases,—considerably less than *one-fourth;* and of the 79 cases examined by Chomel, there were 56 in which no cause could be ascertained (*a*). Every one who has been connected with a large hospital, must have noticed how often pneumonia springs up during the course of various chronic diseases, while the patients have not left their beds, and have been breathing a warm and equable temperature. Every careful observer, states Swett, must have frequently witnessed cases in which no obvious cause of the disease could be

* These 71 cases were observed by Chomel from the 15th March, 1842, to 10th March, 1843, and are not among those referred to in the table by Grisolle, whose book, on Pneumonia, was published in 1841. The 71 cases referred to formed the basis of a lecture by Chomel, upon which notes were taken by our late lamented friend, Dr. W. B. Skipwith, of Baltimore, who gave them to us. So far as we know, the lecture in question has never been published in this country.

(*a*) Leçons de Clinique Médicale, T. Troisième, 496.

detected; that it is of frequent occurrence at every period of life, and in every quarter of the globe, has been noticed in the *fœtus in utero*, and frequently in new-born children (*a*).

It appears, therefore, to be sufficiently apparent that, in a great many instances, we cannot refer the occurrence of pneumonia to the influence of the sudden impression of cold, "*in any appreciable degree*"; and that, in many instances also, the cause of the disease is inscrutable. There can be no doubt, however, that it can be often attributed to the injurious influence of a low temperature, and to sudden atmospheric alternations; especially if the weather be damp and windy; for, as stated by Professor Draper, a temperature of 50° below zero can be sustained without much inconvenience, if the air is perfectly calm, but not so if there is any wind (*b*). It is thus that we must account for the fact, that three times as many cases of pneumonia are seen in the French hospitals in the first six months of the year as in the last; that such was nearly the proportion in the Pennsylvania Hospital during the year 1840, 1841 and 1842,—39 cases having been admitted in the first six months, 12 in the last (*c*); and that, according to our Army Returns, also, of 70 cases reported, 49 occurred during the cold, and 21 during the warm, half of the year (*d*).

There must, then, be some intimate connection between the more frequent occurrence of the disease in cold weather and the impression of cold upon the body. This impression is often not great enough to be perceived at the moment, particularly if the mind is earnestly occupied; or cold may be only partially applied, which is well known to be specially productive of inflammatory attacks; and, in either instance, the patient may not have any recollection of it subsequently. "I am well convinced from observation," says Swett, "that a draught of cold air blowing upon a person, even it is within doors, is far more apt to produce inflammatory action than a much greater exposure to the cold in the open air. It is much more safe to walk out in a

(*a*) Diseases of the Chest, 79. (*b*) Human Physiology, 185.
(*c*) Note by Dr. Dunglison, in Cyc. Prac. Med,, vol. iii. 593.
(*d*) Drake, op. cit., 853.

stormy day, exposed to the wind and to the rain, than to sit quietly in a warm room, near a crack in the window. Many attacks are, I believe, induced by this unheeded exposure." (P. 81). Nor is this unintelligible; for if we reflect upon the fact that the sudoriparous tubes are estimated to be twenty-eight miles in length, and that seven millions of pores are discharging daily thirty ounces of watery vapor and two ounces of excrementitious matter, we need not be surprised that when the normal play of the functions has been disturbed, and the surface even only partially constringed, by the action of cold, deleterious results should follow, and that internal inflammations should be not infrequent in damp and cold weather, and especially when cold has been applied while there is increased vaporization from the skin. In this way reduced temperature may act, unconsciously on the part of the patient, in producing ordinary simple pneumonia; but it is obvious that the very same thing may happen in inducing "malarial pneumonia" in those cases, spoken of by Dr. Manson, "occurring in previously healthy subjects, whilst sedulously guarded from atmospheric vicissitudes." Indeed, when the physiological relations between the skin and the internal organs have been disturbed by the impression of cold, which, as Dr. Manson has pointed out, is more easily done in malarial subjects, these subjects are especially prone to be attacked,—there being, as he truly states, "a degree on the thermometric scale, equivalent to disease with them, yet compatible with health in others."

En passant, we will remark, while on this subject, that whether pneumonia occurs in its simple or the "malarial" form, it is the more likely to be induced by cold, and the perturbating influence of sudden atmospheric changes, when the system is in an enfeebled or exhausted condition from excessive fatigue, and particularly during the state of sleep, which facilitates the operation of depressing agencies. "Wo to him," says Broussais, "who, after a laborious march, is chilled by a cold moisture during the moments of his repose! Instead of deriving new strength from it, the germ of death will be implanted in him" (*a*).

(*a*) Chronic Phlegmasiæ, vol. i. 125.

But while we cannot fail to recognize some close relation between the greater prevalence of pneumonia during the cold, than during the warm seasons of the year, and the impression of cold upon the body; yet we must not conclude, as reasoning, *a priori*, we would be apt to do, that this relationship is in direct proportion to the severity of the cold, and that the disease is more prevalent where the winters are most violent. For it has been shown by the Army Returns of the United States that, while catarrh and bronchial affections diminish as we traverse the centre of the continent from north to south, pneumonia, pleurisy, etc.,—diseases hitherto supposed so nearly allied in their origin, — become more frequent as we advance south — being more common where the seasons are violently contrasted. Hence it was, from the observation of this fact, that our lamented countryman, Dr. Forry, sought to deduce the law, that in proportion as the high temperature of summer makes an impression on the system, do the lungs become susceptible to the morbific agencies of the opposite seasons. Dr. Manson, perhaps, would explain the matter by invoking the agency of *malaria*, which, he thinks, predisposes to the development of pulmonary engorgement. But that influence cannot be as great as such an hypothesis requires; for, as Dr. La Roche has observed, inflammatory pulmonic affections are invariably less prevalent in the moist and changeable climate peculiar to the sea-coast and large lakes of this country, where malaria is abundantly evolved, than in the dry atmosphere of the opposite locality, where the febrile poison is not largely produced; and, in confirmation of Dr. Forry's conclusions, Dr. Lawson, of the English army, has proved that, among the British troops scattered over the greater part of the earth in different stations, the frequency of inflammatory diseases of the lungs is proportional to the prevalence and activity of the (not malarial poison, but) refrigerating causes relatively to the powers of resistance of the constitution (*a*). Moreover, if the malarial theory be sufficient to solve the problem in question, there ought to be a constant relation between the severity of the cold—the

(*a*) Op. cit., 348.

exciting cause — and the prevalence of pneumonia; but Doctor Manson assures us that the *spring*—when the rigors of winter have abated — is the season in which "malarial pneumonia" is "*usually* prevalent."

In regard to the quotation which Doctor Manson makes from Lænnec, ascribing the *epidemic* prevalence of pneumonia to deleterious miasms in the atmosphere, (since, says he, we meet with cases among persons confined to their chambers, in spite of the utmost care taken of their health, and for which we can assign no occasional cause), it need hardly be observed that, whether the reason assigned by Lænnec be true, or not, in respect to some other aerial poison, it cannot be true of the special poison of periodic fevers. For if the malaria that gives origin to these fevers could, *per se*, induce pneumonia, independent of some other state of the atmosphere, would it not prevail most in those seasons of the year during which the evolution of this poison is most active? And so far as the quotation from Lænnec may have been designed to have reference to "*malarial* pneumonia," we would ask, If ordinary simple pneumonia may occur, as we have seen it often does, without our being able to trace it to any appreciable occasional cause, is there any reason why the "malarial" form may not likewise exist, without any evident exciting agency?

It may be safely stated, that the cause or causes which render pneumonia epidemic, independent of those connected with the cold seasons and atmospheric vicissitudes, are not well understood. And, making all due allowance for the influence of the latter, we are often wholly at a loss to account for its greater prevalence in some years, than in others, when, too, affections of a kindred character present no such epidemic tendency: for it has been shown by Ziemssen, from a comparison of the mortality of other diseases with that of pneumonia, that the curve of pneumonia is by no means concentric with that of other inflammatory diseases; nay, on the contrary, that exquisite pneumonic years may be characterized by a rarity of other inflammations; and more singularly still, that there may be an entirely local epidemic of pneumonia amongst children, of whom 22 per cent.

above the annual average may die, while, in unison with every other place, the mortality amongst adults, from the same cause, may be 17 per cent. below the average (*a*). But, as Doctor La Roche has well remarked in respect of epidemic pneumonia, "the diffusion of the disease over large tracts of country; its prevalence at diversified seasons of the year, under diversified conditions of the sensible qualities of the atmosphere; its visiting with equal force localities of different or even opposite characters and conditions; its appearing, even at sea, far beyond the reach of any possible source of exhalation, are inimical to the theory of its *miasmal* origin," and would seem to point to some special, although inscrutable state of the atmosphere. We can readily understand, however, that, if this peculiar and inapprehensible state of the atmosphere should exist soon after an extensive prevalence of periodic fevers, while the malarial poison is still lying dormant in the system, ready to be called into activity by any adequate exciting cause, we may witness the epidemic reign of the disease under notice. Or, as every one must have observed, if pneumonia should be strikingly frequent, and yet not sufficiently so to be properly regarded as epidemic, (and, according to the researches of Ziemssen, it is remarkably characteristic of pneumonia that one culmination point occurs in most places every third year), and this should supervene soon after an extensive prevalence of malarial fevers, we may then meet with an unusual number of cases of "malarial pneumonia."

Finally, in estimating the causes that produce pneumonia, of whatever form, and whether sporadic or epidemic, we should not overlook the influence of that internal disposition which is laid in the organization, the nature of which, Chomel remarks, escapes us, but the existence of which does not admit of doubt. It is on account of this peculiar predisposition, that the disease is so easily excited in some persons by causes that are inoperative upon others, and that it so often re-appears in the same individuals. Dr. Rush treated a German twenty-eight times with it; Andral relates a case in which it recurred the sixteenth time

(*a*) The Virginia Medical Journal, vol. xii. 42, 43.

within eleven years; Chomel alludes to its recurrence the tenth time in the same individual; Dezoteux treated seven times a pneumonia in a subject who had suffered from it fifteen times (*a*); Professor J. K. Mitchell treated one person seven times, and succumbed, himself, alas! to a second attack.

We have now concluded our review of Dr. Manson's essay in regard to his account of the *Symptoms, Physical Signs, Diagnosis, Cause and Nature* of his "malarial pneumonia." Let us briefly recapitulate the main points discussed, and define the positions we have endeavored to establish.

While canvassing Dr. Manson's description of the "*General Course and Progress*" of the disease under notice, we saw—

1. That in simple, and "malarial pneumonia," alike, the *invasion* is preceded by *prodormata* in a certain proportion of cases, attested by the phenomena of general febrile disturbance, or of pulmonary catarrh, varying in duration from one to several days before the appearance of any local symptom of the affection.

2. That in simple, and "malarial pneumonia," alike, the onset is *sudden* in the vast majority of cases, and heralded by a *chill*, usually of marked intensity, and of variable duration.

3. That in simple, and "malarial pneumonia," alike, *pain in the side* follows close upon the *chill*, constitutes a prominent symptom, and is present in an immense majority of the cases.

4. That in simple, and "malarial pneumonia," alike, *dyspnœa* and *cough* generally soon succeed the fever of reaction, and vary greatly in their intensity, without offering anything distinctive of the one from the other.

5. That in simple, and "malarial pneumonia," alike, the *expectoration* presents great diversity in its character and appearance; but that the thin, slightly tenaceous, bloody expectoration which Dr. Manson deems diagnostic of the "malarial" form, is met with also in cases (chiefly of an asthenic type) presenting no indications whatever of a miasmatic element, and, on the other hand, is absent in many cases—much the larger number,

(*a*) Cyc. Prac. Med., vol. iii. 593.

according to our experience—in which this element is most conspicuous.*

6. That in simple, and "malarial pneumonia," alike, *cephalalgia* is a very frequent and prominent symptom.

7. That in simple, and "malarial pneumonia," alike, *nausea*, and the *vomiting* of bilious matters may occur; but that they are much oftener present, and much more prominent, in the "malarial" form.

8. That in simple, and "malarial pneumonia," alike, as they occur at the South, the *intestinal mucous membrane* evinces greater susceptibility to irritants than it usually exhibits in colder latitudes; but that this tendency is not peculiar to, or in any manner diagnostic of, a malarial diathesis.

9. That in simple, and "malarial pneumonia," alike, convalescence is not usually announced by a particular kind of *inodorous stools.*

10. That in simple, and "malarial pneumonia," alike, the character of the *febrile movement* may be evidently remittent; but that, generally, this character of the fever is much more marked in the latter, and constitutes its most prominent distinctive feature.

11. That in simple, and "malarial pneumonia," alike, the condition of the *pulse* depends upon the character of the disease, whether this be of sthenic or asthenic type; but that, at the present time, its character may be regarded as, usually, indicative of a *status* intermediate between the two. Yet, that, since the grade of inflammation is habitually less elevated in "malarial," than in ordinary simple pneumonia, the pulse, in the former, generally betokens a diathesis less exalted, without offering, however, anything specially diagnostic of a miasmatic element, inasmuch as, in *typhoid* pneumonia, *par excellence*, the pulse is soft and compressible almost always.

* It seems to us that, in his "*Synopsis of the termination in health*," Dr. Manson makes a statement which is contradictory of the one representing the thin, slightly viscid, bloody expectoration as the variety "*more frequently* observed in "*malarial* pneumonia." In a Note to his account of the Physical Signs of the Second Stage of Solidification, he assures us that the expectoration under consideration is an EXHALATION, and *not the result of inflammation;*" and yet, in the

12. That in simple, and "malarial pneumonia," alike, *delirium* is of frequent occurrence; but that, neither by the frequency of its occurrence, the time at which it appears, nor by its character, does it offer any special indication of the "malarial" form. Indeed, so far does its supervention late in the career of this variety of pneumonia fall short of constituting one of its diagnostic features, as Doctor Manson has affirmed, that Trousseau represents it to be of habitual occurrence in fatal cases of pneumonia; for, says he, in speaking of a case that terminated unfortunately—"I need not add that the man was *delirious* during the last two days of his life, which is *always the case when death is inevitable from inflammation of the parenchyma of the lungs*" (*a*).

13. That in simple, and "malarial pneumonia," alike, *relapses* are not very infrequent during some period of convalescence.

14. That in simple, and "malarial pneumonia," alike, the disease is characterized by well-defined *physical signs*, in the immense majority of cases; and that there is no satisfactory evidence that, in this respect, they offer any essential difference.

15. That in simple, and "malarial pneumonia," alike, *post-mortem* examinations show that the inflammation almost always presents the *lobar* form,—ordinary *lobar pneumonitis*,—characterized by the usual products of the inflammatory process, when seated in the pulmonary parenchyma.

16. That in simple, and "malarial pneumonia," alike, the disease is most common during the colder months of the year, and, in the majority of instances, is referable to *causes* connected with the action of cold and atmospheric vicissitudes; these, when they operate upon constitutions previously impressed by malaria, arousing into activity that latent poison, and producing

Synopsis referred to, he describes the sputa as gradually assuming the smooth straw-colored appearance, *characteristic of the resolution of* INFLAMMATION"! Now, if the expectoration in question is an *exhalation*, and *not* the result of *inflammation*, and is "more frequently" met with in "malarial pneumonia" than any other, how does it happen that, in portraying the phenomena of resolution, the sputa are represented as *habitually* passing through the phases that are "CHARACTERISTIC *of the resolution of pulmonic* INFLAMMATION"? One or the other of these statements is not correct.

(*a*) Virginia Medical Journal, vol. x. 488.

a complicated disease under consideration; but that, in both varieties of pneumonia, the disease is sometimes observed, when it is impossible to trace it to any evident exciting cause.

17. Lastly, that this complicated disease has been described by many authors, both ancient and modern, and has been clearly set forth in the writings of those of recent date, and of easy access; so that no physician can justly plead ignorance of it on the score that it "has not attracted the attention of recent investigators, and is only cursorily and imperfectly noticed by any of the modern standard authorities," as Dr. Manson has so bluntly affirmed.

It is now for the reader to determine whether we have produced sufficient evidence to maintain these positions; and whether we greatly erred in stating, in the beginning of this Review, that what Doctor Manson considers as a separate and distinct form of pneumonia, is in the immense majority of cases, at least, nothing more, as regards its *essential nature*, than ordinary *pneumonia*, of asthenic or sthenic type, accidentally impressed by a malarious influence. Indeed, in every instance in which (so far as we can learn) the pathological condition of the lungs has been determined by *post-mortem* examinations, (excepting the two cases of *lobular* pneumonia referred to, one observed by Swett, and the other by Morehead), the lungs have presented the usual products of inflammatory action, as in simple uncomplicated *lobar pneumonitis*, and in no case whatever have those examinations revealed anything at all peculiar to, or in any manner distinctive of, the "malarial" disease. It is very true that, in portraying the symptoms of "*bilious pneumonitis*," Drake states that the functional disturbance of the lungs "results largely from sanguineous engorgement, and retarded transmission of blood through the organ," (and this may be admitted without giving up the doctrine that the solidity of the second stage is due to *inflammatory exudation*); still, he takes care to state, also, that the pulmonary lesion "consists in *well-developed inflammation*"; that is to say, although "the malady under consideration is a pulmonary *inflammation*," yet that the signs of an *intense* phlogistic

action are less conspicuous than in ordinary pneumonitis" (*a*). Constant, to whom we have already referred in another connection, goes much farther, and asserts that, in the red *hepatization* of the second stage of the intermittent pneumonia, the engorgement is rather a sanguineous congestion than inflammation (*b*); but he has not fortified this statement by the positive proofs which, in our time, Science demands in the determination of a pathological problem. At present, we are justified in claiming that this doctrine must first be *proved to be true*—and, too, by such tests as will be admitted as conclusive by the best modern pathologists—before it can challenge our credence. The reader will remember, in this connection, that Doctor Manson denies that, in "malarial pneumonia," the lungs are *hepatized* at all.

But while we cannot admit that, in "malarial pneumonia," the pathological condition of the lungs is "widely different" from that in simple uncomplicated pneumonia, as regards the *essential nature* of the lesion; yet, in respect of successful practice, it is highly important that we should be able to recognize, and to interpret correctly, the vital phenomena by which the blending of a malarious element, with the ordinary pneumonia of the country, is indicated. Before concluding this part of the subject, therefore, we will briefly recur to some of those symptoms which Doctor Manson deems diagnostic of the disease under notice, and endeavor to point out more fully, than we have done heretofore, those upon which, in our judgment, we must chiefly rely.

1. *Marked remittent and paroxysmal character of the fever.*

Undoubtedly, the most constant, most prominent, and, therefore, the most reliable distinguishing characteristic of pneumonia impressed by the poison of malaria is, the marked periodical exacerbations and remissions of the febrile movement. It is very true, as we have previously stated, that, in simple uncomplicated pneumonia, the fever may be decidedly remittent and paroxysmal; and we believe, with Morehead, that there is often a relation between the frequency of the occurrence of this type

(*a*) Principal Diseases of the Interior Valley of North America, *second series*, 896.

(*b*) Ranking's Abstract, No. xviii. 74.

of fever and asthenic states of the system; but the fever is rarely as decidedly remittent aad paroxysmal as it is *habitually* in pneumonia connected with a malarial diathesis. The course of the fever, in cases of this character, both when they present the *intermittent* and *remittent* forms, is so admirably described by Grisolle (see April number of this Journal, p. 430), as to admit of little qualification or amendment. We would state in addition, however, that, in pneumonia impressed by a malarial influence, the daily exacerbations of the fever are, in a great many instances, accompanied by the phenomena that are generally so prominent in the ordinary forms of periodic fever; that is to say, besides the increased heat of skin, accelerated pulse, and more urgent thirst, the febrile access is announced by returning headache, pains of more or less violence in the small of the back, and the limbs, especially an aching in the legs, and, perhaps, nausea,—if these symptoms had subsided during the remission of the fever; or, if they had merely abated without ceasing entirely, they now become more prominent and distressing, and continue until the subsidence of the fever, when again they are less severe, and disappear entirely; and as the intensity of the fever diminishes, the skin relaxes, and more or less perspiration betokens the coveted period of remission. The first few exacerbations of the fever are habitually heralded by a *chill*, of more or less intensity, each successive one becoming less and less obvious, and which may not exceed a marked coolness of the tip of the nose, the ears, and the ends of the fingers and toes, with some blueness about the nails; but, unless the associated type of periodic fever be of the *intermittent* form, it is not possible, according to our experience, that the evidences of a *daily* chill can be "*almost invariably observed*," as Dr. Manson has affirmed. And in this we are sustained by the observations of Doctor L. H. Anderson, who states that, in the *remittent fever* which accompanies the pneumonia of South Alabama, there is usually no chill after the *first* one, or, if any, it is so slight as not to be noticed (*a*). It is very true, however, as Dr. Manson says, that marked coolness

(*a*) N. O. Med. and Surg. Journal, vol. xi. 78.

of the skin, which may be confined to the extreme parts, may indicate the presence of a chill to a bystander, although the coolness be not felt by the patient. In the absence of the chill, it is by noticing a marked display of periodicity—a peculiar acuteness and distinctness — in the *febrile phenomena* we have described, that the complicated character of the pneumonia may be most certainly ascertained; and in proportion as they are prominent and easily recognized, is the diagnosis rendered sure and easy. In some instances in which the element of periodicity is feebly engrafted on the system, the remissions are obscure, as Doctor Manson has stated, and an ardent fever exists during the whole day and night; still, even in these cases, we may often detect, upon close inspection, some decided abatement of the febrile movement, early in the morning; and the returning violence of the pains in the head, back and limbs, and disposition to nausea, easily provoked by medicines that are generally well borne in simple frank pneumonia, in connection with slight signs, perhaps, of bilious deterioration, admonish us of the subtle working of the latent poison of malaria. If, in cases of this doubtful character, the bilious phenomena be more or less conspicuous, we are aided in the diagnosis in proportion to the degree of their development.

The fever is usually of the quotidian type; much more rarely, it is of the tertian form; and only once have we seen pneumonia complicated by a *quartan* intermittent, a form which must be excessively rare, since Grisolle could find no example of the kind in his extensive researches. The *remissions* in the fever almost always occur in the morning, if we may judge from our own observations.

2. *Nausea, bilious vomiting, and an icteroid tinge of the tongue, eyes, skin and urine.*

These phenomena are certainly very conspicuous in a certain number of cases of pneumonia connected with the diathesis of periodic fever; but they are absent in many cases, as they occur in this latitude at least, obviously impressed by a malarious influence. They are not mentioned by Grisolle, whose description of *Intermittent and Remittent Pneumonia*, in most respects

so excellent, is drawn from the writings of many authors, both ancient and modern, scattered through the annals of science.*

The symptoms, under consideration, are not, generally of equal prominence. *Nausea*, if persisting, is a very valuable correlative symptom, and especially if it is most evident during the exacerbation of the fever, and subsides, or entirely disappears, as that remits, to be excited again during the febrile access. When associated with a sense of fullness and tension in the epigastrium, with marked tenderness upon pressure, it derives additional importance. *Bilious vomiting* occurs in the mass of cases attended by abiding nausea: and, in some instances, we have known a large amount of bile ejected, and accompanied, occasionally, by bilious dejections. In other cases, the liver, from over-stimulation, fails to secrete, in obedience to the well-established physiological law, that exhaustion soon succeeds to excessive stimulation; and, then, the nausea and vomiting may be distressing, and yet productive only of gastric mucus, and the fluids recently swallowed. In either class of cases,—both when the nausea is, and is not, attended by bilious vomiting,—there is often a painful sense of fulness, and some tenderness upon pressure, over the right hypochondrium. The congestion of the liver, thus indicated, is often relieved, by the action of a mild emeto-mercurial cathartic: and the gastric and hepatic troubles may then greatly subside, or entirely vanish. The *tongue*, in the cases under consideration, is generally stained with a yellow or greenish yellow coating, more or less marked; in some instances, it is of a deep saffron color; in others, there is only a slight yellowish tinge, and this may be the only evidence that the liver is not acting normally and duly eliminating the biliary pigmentary matter from the blood. The tongue is generally

* Some may offer captious objections to Grisolle's account of the disease under consideration, on the ground that it is not based on his personal experience. As a whole, however, it is one of the best that we have seen; and as it a *résumé* of the writings of various authors, whose observations were made at different times, and in different localities, it is not to be lightly esteemed. It is for this reason, that we have repeatedly referred to his statements in respect to the frequency with which certain signs and symptoms are met with, in the affection under notice.

moist in the earlier stages of the malady, and especially so during the remission of the fever: later, if the case progresses unfavorably, it becomes dry, and of a reddish, dark brown, or faintly blackish hue, with a filthy collection of sordes around the gums, teeth and lips. In one instance only, have we noticed a deposit on the teeth, just along the margin of the gums, of a light orange color, as described by the late lamented Doctor Boling, of Alabama, and which he suspected might be developed by miasmatic poisoning of the system (*a*). The *eyes and skin* are tinged yellow in a certain number of the cases, under contemplation; and the patients occasionally complain of a sense of soreness in them, especially in looking at objects sideways, and attended by a tensive pain across the forehead; and the face, when flushed, as Potter aptly remarks, presents "a sickly mixture of yellow and red" (*b*). The *urine* is frequently of a deep yellow or dark brown color, from the presence of bile, or its coloring matter,—showing that the hepatic cells are inactive, and that bile does not flow freely into the duodenum; and the serum discharged from blisters may be yellow from the biliary impregnation of the system.

3. The *Sputum.* When speaking of the expectoration, (see February number of this Journal, for 1859, p. 318), we stated that Andral had shown that its characteristic appearances in pneumonia are due to the varied intermixture of mucus and blood. The expectoration in pneumonia, however, using the word in a comprehensive sense, presents, microscopically, a diversified appearance, and hence, strictly speaking, is much more than mere mucus and blood commingled, or the inflammatory exudation which fills up the air-cells, and gives rise to those numerous granulations offered by the torn or cut surface of hepatized lung. Without entering fully into the matter, we will simply state, that there seems to be good reasons for the belief, that the sputum is sometimes tinged by the coloring matter of the bile. Andral was far from wishing to deny the influence of this cause in modifying the appearance of the sputum; and, in

(*a*) American Journal Medical Science, N. S., vol. xxiv. 278.
(*b*) Baltimore Med. and Surg. Journal and Review, vol. i. 266.

proof of its agency in this regard, he gives a case; and Grisolle, Chomel, Martin, Solon, Potter, Drake and Swett, make similar statements. It is well known that when, from any cause, biliverdin, the pigmentary principle of the bile, is not eliminated from the blood, the skin, the conjunctival and lingual mucous membranes, and the urine, offer a yellow hue, owing to the accumulation of the biliary pigment in the epidermic cells, the uriniferous tubes, etc. Is it not reasonable to suppose, then, that, under similar conditions, the *sputum* may be tinged in like manner? But as malarial poison is not the only cause of a similar disturbance in the functions of the liver, the sputum, thus colored, cannot certainly denote the action of a malarious element; and hence, as we have stated already in another connection, there is no special character of the sputa *peculiar* to, or *diagnostic* of, that variety of pneumonia under consideration; still, as in this latitude, the liver is oftener deranged through the operation of miasmatic poison than from any other cause, the sputum, when colored by the pigmentary matter of the bile, oftener occurs in connection with the manifestation of the influence of this poison than it does independent of it, and in this qualified sense, therefore, it may lend some help to diagnosis. And this is the only character of the sputum, which, according to our experience, in any manner or degree, reflects the impress of a malarial law; and even this modification of its usual characters, is witnessed in only a very small proportion of cases of pneumonia, unmistakably connected with a malarial element. Yet, we have seen it, when there were no other evidences of inactivity of the hepatic cells, although, for the most part, it is associated with coincident tokens of hepatic torpor, and especially with a bitter taste of the buccal secretions, and a marked yellowish coating of the tongue. The color of the sputum in question varies from a deep yellow, reddish-orange or gamboge, to a dark brown, or tobacco-juice appearance.

To resume: If the fever be of a *marked remittent and paroxysmal character*, and attended by cephalalgia, and pains in the lower spine and legs, that uniformly recur with the exacerbations, and greatly subside, or pass off entirely, during the remis-

sions; and, especially, if the febrile movement elapses with diaphoresis, more or less diffused; if the *stomach* be abnormally irritable, or very easily nauseated by the usual remedies (ipecac. and antimony) that, in one form or another, are nearly always employed in combination with other agents; if the bilious phenomena described be more or less conspicuous; these constitute the aggregation of symptoms which should arrest attention, and determine the infusion of a miasmatic element into the case before us. But while these broad lines of discrimination, between simple and "malarial pneumonia," may be readily drawn in some cases, yet, in many others, they intersect each other at various points, or approach so nearly as to become commingled, and no longer separately visible: so that it is exceedingly difficult to recognize them, and to distinguish between the ordinary and the complicated affection. We can, therefore, in this respect, heartily endorse Dr. Manson's statement, that "although the rules laid down are generally applicable, we should ever be on the alert in fear of a mistake." It often requires much clinical tact, and an extended experience, to enable one to detect, beneath the mask of symptoms announcing the pulmonary inflammation, the subtle play of the latent malarial element which complicates, and enhances its danger.

The pneumonia and periodic fever complication may (1) start together, and pursue an even march; or (2) the pneumonia may set in as in the ordinary way, and it is only as the disease advances, that we witness any display of symptoms announcing the malarial diathesis; or (3) the pneumonia may be only an intercurrent affection, complicating the career of one of the forms of periodic fever. The latter occurrence, if we may judge from our own observations, must be very infrequent in this section of country.

4. *Physical signs.* In pneumonia impressed by a malarious influence, we have never discovered any peculiarity in the *physical signs.* If the type of the disease be of marked asthenic character, and the prostration extreme, the physical signs are not so well declared in the majority of cases of pneumonia, whether there be a malarial complication or not. In the first

stage of asthenic pneumonia, the *crepitant ronchus* is usually more rare, more humid, and the crepitations are larger than they habitually are in sthenic pneumonia, and very much resemble the *sub-crepitant rhonchi*, with five bubbles, of MM. Barth and Roger; but the crepitant rhonchus occupies a smaller space, explodes in distinct puffs under the ear, with great rapidity, and is less continuous, and more equal in size, than are the bubbles of the *sub-crepitant rhonchus*. If the adynamia be considerable, it is necessary to request the patient to breathe deeply, or, in many cases, the crepitant râle may be overlooked. This râle does not exist in all cases of pneumonia; that is, we have not heard it in all cases. But a large majority of our cases, occurring out of the town of Warrenton, are not seen until the 3d or 4th day, and then only once in 24 hours, when the period of the existence of the râle has passed by. We have not noticed its absence oftener in malarial, than in non-malarial, cases: at least this is the impression which quite an extensive experience has left on our mind. Whether it be that the intra-parenchymatous exudation is thrown out so rapidly as to arrest all breathing action in the really inflamed part, *ab initio*, as Walsh suggests, or not, certain it is that we have not observed primary crepitation in some patients, affected with pneumonia, that have been watched with great care; and still oftener we have not been able to trace the return from the *second* to the *first* stage, by the occurrence of *crepitans redux*. In all this, however, there is certainly nothing peculiar to "*malarial* pneumonia." Still, we regard true crepitant rhonchus as *the physical sign of the first stage of lobar pneumonia;* and know not, if this be absent, what other physical sign indicates this stage. We are very well aware that *feeble respiration*, with loss of purity and softness, not unfrequently precedes the appearance of the crepitant rhonchus; and in some cases, the respiration is rough and rustling; but although we may sometimes predict the advance of pneumonia by observing these modifications of the respiratory murmur, yet they are not noticed sufficiently often to afford any very reliable indication, and are, besides, much oftener observed in simple bronchitis, etc.—The physical signs of the *second stage* of pneu-

monia, we have not had any difficulty in recognizing, save in some few cases of central pneumonia, in which the disease evidently occupied a small space. In some very adynamic cases—malarial and non-malarial—the bronchial respiration is feeble under ordinary respiration; and it becomes necessary, in order to elicit it, that the patient be directed to breathe faster and stronger. That neither bronchial respiration, bronchophony and bronchial cough—the almost constant signs of hepatized lung—could be observed by Doctor Manson in a large majority of the cases of pneumonia that he has witnessed, is to us, we must confess, a very surprising and most incomprehensible statement to be made by any one, sufficiently expert in physical diagnosis, to justify his writing about the subject at all.

In conclusion, we would again direct special attention to the important fact, that pneumonia, like other diseases, occurring in a malarious region, may run its course without the slightest evidence of a malarial complication; and after the subsidence of every serious symptom, we may find the patient unexpectedly growing worse, while the most careful physical exploration reveals no increase of pulmonary disease. If the physician is on the alert, detects the true nature of the new element, which, as Grisolle says, has come to join itself to the primitive affection, and acts promptly, the patient is saved. Otherwise, death may ensue in the next paroxysm. There will be a quick death, or a speedy victory.

PART IV.

TREATMENT.

Under this caption, Dr. Manson has the following:

In the Chill.—In this stage it is evident that the state of internal congestion is most intense; the indications are therefore to remove or lessen this condition. As in ordinary cases the vital powers are competent to produce reaction, the employment of active measures will be unnecessary, but when the cold stage is serious and protracted, the usual means adopted in remittent fever should be called into requisition. "Warm applications are to be assiduously made over the whole length of the spinal column and to the extremities, together with stimulating frictions," and to these may be added warm cataplasms of mustard or capsicum to the anterior and lateral surfaces of the thorax. If these means should be unsuccessful in restoring

the circulation to the surface, enemata of the sulphate of quinine may be used: eight or ten grains of the salt dissolved in two ounces of mucilaginous fluid may be thrown into the rectum every half hour, or more often, until symptoms of reaction ensue. If diarrhœa has been or is present, the following enema will be more efficacious:

Quinæ sulph., grs. x;
Morphiæ sulph., gr. ½;
Acid. sulph. dilut., gr. x;
Aquæ, oz. 2.
M.

To be used as the former, but at longer intervals.

The action of the enemata may be assisted by the administration of carbonate of ammonia by the mouth, or infusions of ginger or capsicum whilst warm may be given. In extreme cases it will be necessary to resort to alcoholic stimuli. These, however, should be used with caution, and their effects closely observed.

Fortunately, however, it is rarely that much difficulty is encountered in conducting the patient through the cold stage, and medication may be generally postponed until the febrile reaction is developed.

The Exacerbation.—The indications in this stage are to calm the action of the heart, to remove congestion, and to prevent the occurrence of the next paroxysm. If the pulse is hard, full, or tense, as it is in rare and exceptional cases; or if the patient is robust or previously healthy, and possessing ordinary vigor, and the pain or dyspnœa is very intense, and the character of the pulse or other symptoms do not decidedly contra-indicate its employment, then a moderate quantity of blood may be taken from the arm. In this disease general blood-letting should be employed with a view *only* to *moderate*, and not with an expectation to cut short the disease, nor should it be employed except in the instances referred to. In cases where this is not followed by marked relief, and in those where venesection is inadmissible, the local abstraction of blood by cups or leeches should be resorted to, and may be used as freely as considerations of safety will permit. The time at which bleeding* by any mode should be practised, is that period when the exacerbation has reached its acme, which is almost invariably in the afternoon or evening.

The administration of medicine is generally postponed until nine or ten o'clock, P. M., at which time a full dose of calomel (fifteen to twenty grains) should be administered. If diarrhœa, or a tendency thereto, is present, eight or ten grains of Dover's powder are combined with the mercurial. Two or three hours thereafter, this is followed by a full dose of quinine (fif-

* In the remittent fevers denominated pernicious peri-pneumonic, says Lænnec, it may no doubt be sometimes necessary to bleed during a paroxysm, in order to prevent suffocation, but the utmost caution is requisite not to destroy unnecessarily the strength of the patient. We must ever keep in mind that blood-letting cannot cure a disease which will certainly return after a few hours with fresh violence, and of which experience has long since demonstrated bark to be the only remedy.—Op. cit., 253.

teen to twenty grains), in pills, or diffused in a wineglassful of cold water. Three or four hours are now allowed to elapse: we now usually repeat the quinine in doses of five to ten grains, in proportion as the pulse may have been reduced, until thirty-five or forty grains have been administered, and the patient is then left at rest.

According to my experience, the chill cannot recur whilst the patient is under the influence of the above quantity of the alkaloid; the fever may, however, continue, but usually in a very moderate degree. Should this be the case, the local bleeding is repeated in the evening exacerbation, and calomel, in small doses of one or two grains, with a half to one grain of ipecac., or two grains of Dover's powder, is given every one or two hours. After midnight the quinine is repeated in the same manner, or in lesser quantity (in direct proportion to the degree of fever present), until the period of chill has passed, when all medicine is again suspended. It is rare, indeed, that cases thus treated in the early stages do not exhibit the most flattering amendment on the day succeeding the first night on which the plan is adopted; and still more rare, that every dangerous symptom has not disappeared on the succeeding day. Should the pneumonic symptoms, however, persist, the mercurial should be continued until its constitutional effects are produced. In some cases, where the arterial action is not disposed to yield, and the dyspnœa continues, I have ventured on the use of tartar emetic in small doses, in union with quinine. I am indebted to Doctor Blakiston for a knowledge of the value of this combination. The antimonial should, however, be carefully used; and it is better to commence with a minimum dose of one-tenth or one-twelfth of a grain, to be gradually increased as it may be borne. I think it proper to say, too, that the gastric irritation and tendency to diarrhœa, so often present in this disease, contraindicate the employment of tartar-emetic, generally. Whether alone, or in combination, the quinine should, however, be continued for several days after the active symptoms have disappeared. Blisters may be employed at almost any stage of the affection, but under this mode of treatment may be often altogether dispensed with. Of course their use is preferable after the arterial action is moderated.

We could say more, but we have already, we fear, outwritten the patience of the reader. That which we would add, would, however, be chiefly an extension of our views in regard to the use and action of quinine. But mark the prophecy! Howsoever much may have been written, the category of diseases has only been commenced, which yet will yield to its sway, whose destiny is not only to remove, but to unveil, perhaps, the essence of morbid action itself. Mysterious, wondrous, potent gift to humanity, more precious than the gold or gems of the sunny land beyond the burning zone, decked by thy imperial flowers! Minister and missionary of nature! Science places an illuminated crown upon thy snow-like brow, and proclaims thee Sovereign Queen of the vast medical realm! Of old the Magian would have bent in adoration before thee, whose powers, surpassing the results of reason and the inspirations of genius, eclipse in certainty, celerity and brilliancy, the fabled incantations of magic!

I. *Treatment in the Chill.*

Doctor Manson's directions for conducting the patient safely through the chill, are, we think, very judicious, in the main. But as, in the "malarial pneumonia" of this region, active interference is seldom required during this stage of the disease, its consideration need not detain us long. The treatment varies little from that usually employed in the chill of the simple forms of periodic fever. In addition to the means enjoined by Doctor Manson, if the pulmonary oppression were considerable, we should be disposed to administer an emetic—the good old-fashioned one of salt and mustard,—if the stomach were not irritated, as it acts quickly and without enfeebling the forces; or, an emeto-cathartic of ipecac. and calomel, if gastric irritability should contra-indicate a stimulating emetic, and especially if there were any indications of hepatic disorder. Great caution is necessary, however, in giving emetics in every form of disease impressed by the poison of malaria; for it has too often happened that a mild form of miasmatic fever has been converted into the pernicious variety, by the injudicious employment of emetics. In the circumstances under consideration, we should never think of giving *tartar emetic* for the purpose of favoring reaction, as advised by Dr. Samuel A. Cartwright in his Essay on *Pneumonia Biliosa*, which, although published a third of a century ago, seems but recently to have found much favor in a certain quarter. Even when pneumonia occurred in connection with *algid congestive fever*,—with great irritability of stomach and loss of strength, dry and black tongue, the surface of the body being cold, especially the extremities, the skin of the hands and fingers shrivelled like those of washerwomen, pulse at the wrist small and weak, with violent determination of blood to the head,—Dr. Cartwright gave, with the view of restoring heat to the cold, torpid and insensible surface, *tartar emetic* in 8 and 10-gr. doses; and, in one instance, actually administered "*rather more than* TWO DRACHMS *in thirty-three hours*"!* If the chill be profound,

* *The Medical Recorder*, vol. x. 41–44, 61.—Lest his readers might suspect that the *tartar emetic* used was impure, Dr. Cartwright, very thoughtfully, adds: "The tartar emetic used in the above case was genuine; I used the same kind in my

and protracted, we may derive great benefit, in congestive fever, from the application of a large, hot, cayenne or mustard poultice over the epigastric and hypochondriac regions, to be succeeded, if necessary, by a large blister, moistened with hot spirits of turpentine. These are remedies of great power in quieting the irritable stomach, spurring up the flagging forces, and determining to the surface. If the extremities be cold, clammy, shrivelled, and exuding a profuse watery fluid, persevering frictions with dry mustard should never be omitted. But, in cases of this character, *quinine* is *the* remedy. If the stomach be retentive, it should be freely given dissolved in hot spiced brandy toddy, with some preparation of opium. Should the stomach be too irritable to retain quinine in solution, it should be dissolved in simple warm water, (of course with an acid, in either case), and laudanum should be added, unless forbidden by profound coma, and given as an enema. As water is more rapidly absorbed than a mucilaginous fluid, it is the better vehicle. In all bad cases, however, we prefer to give quinine by the mouth, if the stomach can be coaxed to retain it; and even when the gastric irritability is quite considerable, quinine, when made into pills with suitable portions of morphine and creosote, will very seldom be rejected, especially if the epigastrium be plied with counter-irritants. The doses of these remedies should be apportioned according to the exigencies of each individual case

practice, and found it to be equally efficacious in producing vomiting, &c., as any I had ever used; I procured it myself from the apothecary, and accurately weighed every dose. After the patient had taken forty-eight grains, lest there might be some fault in the medicine, I went to another apothecary, and procured a quantity made at a different place, and used this instead of the former. The remaining tartar emetic, in each vial, was afterwards used, and found to produce the ordinary effects of the best tartar emetic." As giving additional proof of the wonderful tenacity of life possessed by the person in question, we will further state that, at one of his visits, Dr. Cartwright ordered his patient—who had, at the time, dry tongue, delirium, inflamed and blood-shot eyes (being badly off in more ways than one)—"ten drops of NITRIC ACID in prickly-pear tea, *every hour and a half through the day*"; and still, Mr. Boerhuss (for this was his name) "*perfectly recovered*"! The effects of tartar emetic, in Dr. Cartwright's hands, were rather unusual, in another respect. "If," says he, "the patient, "after having taken a dose of this medicine, feels like *vomiting*, *another dose now given* will generally *prevent that effect from being produced.*"

—the quinine being given in doses of from 5 to 20 grains, and repeated as may be indicated.

But we have never witnessed this perilous association of pernicious fever at the *onset* of pneumonia, in this section. The few cases that we have observed, in which pneumonia appeared to be complicated by a *congestive chill*, as it is familiarly called, occurred in the most unexpected manner, at an advanced period of the pulmonary disease, when the inflammation was undergoing resolution. Supervening at a time when the system is enervated, this unfortunate complication is particularly dangerous, and requires the most prompt and decided interference on the part of the physician, or death speedily closes the scene.

II. *Treatment in the Exacerbation.* The indications under this head are, Dr. Manson states, to calm the action of the heart, to remove congestion, and to prevent the occurrence of the next paroxysm. These are fulfilled, if the pulse is hard, full or tense, as it is in exceptional cases, or if the patient is robust, previously healthy, of ordinary vigor, and the pain or dyspnœa very intense, by *bleeding*,—so as to *moderate*, and without any expectation of cutting short, the disease; and, in those cases in which bleeding is not followed by marked relief, and in those in which it is inadmissible, blood should be taken by *cups or leeches* as freely as can be safely permitted; and the loss of blood, whether general or local, should be practised when the exacerbation has reached its *acmé*.

However unsound his pathology, and however untenable his views in other particulars, yet, in the circumstances under consideration, when Dr. Manson comes to apply his therapeutics, it is really pleasing to witness how the sagacity of the practical physician shines forth. The indications are clearly stated, the means advised highly judicious, and we take pleasure in commending them to the approval of the profession. The directions enjoined by Dr. Manson are applicable to pneumonia, whether impressed by a malarious influence or not; and embrace the principles upon which the mass of enlightened physicians in this region have based their practice, during the past few years, in

the type of pneumonia now prevalent among us. It follows, therefore, that although, in *Dr. Manson's* opinion, his *confrères* have sadly mistaken the pathology of the form of pneumonia, which, says he, is "generally seen by Southern observers"; and however "often" they have failed to recognize "its peculiarities," and however "often" they have "confounded" it "with simple, typhoid, and other varieties of pulmonic lesion," as Doctor Manson so boldly charged against them; yet, so far certainly as the employment of the potent agent, bloodletting, both general and local, is concerned,—which so greatly taxes the skill of the physician,—they have acted wisely and well, and are *not* amenable to the grave accusation preferred by Dr. Manson, of having committed the "*consummate and fatal folly*" of "*blindly following a treatment intended for varieties of disease they never witnessed.*"

Since the time of Hippocrates, bloodletting has been regarded, by a multitude of physicians, as the *remedium magnum* in pneumonia,—to be used with proper discrimination and judgment; and during all this lengthened period, observing and reflecting men have practised it largely, moderately, or refrained from it entirely, according to the *status* of the vital forces in each individual case. Like everything else, however, bloodletting has been used, in every age, to great excess; and it has often happened that a profuse and preposterous expenditure of the vital fluid has driven some, who witnessed its destructive effects thus practised, into the opposite extreme of abandoning it altogether. Every one knows that, within a few years past, a great change has taken place in the treatment of inflammatory diseases,—especially the practice of bleeding; and that although formerly it was the *rule* to bleed, in such affections, it has now become the *exception*, and is rarely resorted to. This change in practice, admitted on all hands, has been differently accounted for by different observers. We have (1), Professor J. Hughes Bennett and his party, contending that the great revolution in treatment has resulted naturally from the great advances, made in modern times, in diagnosis and pathology; (2.) the late Professor Alison and his party, contending that the type of disease,

having changed from a sthenic to an asthenic character, the practice has very properly changed accordingly; and (3.) Doctor Balfour and his party, contending that the change has originated from neither of these circumstances, but has been forced on all alike by the results of a successful empiricism. Although the controversy involves the treatment of *inflammation* in general, yet, practically, it has turned, almost entirely, on the treatment of pneumonia by bloodletting.

It is beside our purpose to discuss these questions here. Suffice it to say, that, with Kennedy, we believe that both animal and vegetable life is subject at times to epidemic influences, which at one period raise, and at another depress, the standard of health: that pneumonia, like fever, alters its type at certain times, and that no single plan of treatment can, therefore, possibly meet the ever-varying shades of disease—pneumonia among the rest; or, as Watson has expressed it, we are fully persuaded, both by our own observation and the records of Medicine, that there are waves of time through which the sthenic and asthenic characters of disease prevail in succession; and that we are at present living amid one of its adynamic phases. Thus believing, we can have no difficulty in understanding why it was that, of 145 *autopsies* of pneumonia in the hospitals of London, which Doctor Hughes published in 1842, only 11 were gangrenous, or 1 in 13; while of 54 *autopsies* made between 1842 and 1849, 18 were gangrenous, or 1 in 3. As might have been anticipated, however, it has been observed that, in certain places, diseases have not exhibited so adynamic a type as they have in others. In this section, within the past eight or ten years, typhoid fever has, to a great extent, supplanted other fevers of a more elevated grade, and become *the* fever of the country; and during the reign of this typhoid dynasty, so to speak, other diseases have presented an asthenic type, more or less marked, and have not required general blood-letting, save in exceptional instances. Indeed, it appears to us that we are in no little danger of eschewing it altogether; and some are already possessed of the idea that, if not positively hurtful, it is at least nugatory for good, in pneumonia, and exercises no favorable influence upon

the march of the disease. Although we are well aware of the danger of appealing to statistics in the treatment of disease; that statistics cannot furnish us absolute guides, since so many modifying circumstances are perpetually arising to set at defiance all positive rules, and that, therefore, every case should be treated according to its own individual peculiarities; yet when cases are selected on account of their resemblance in all respects as near as possible, we may often study to advantage the influence of remedies upon certain symptoms, and upon the march and duration of the disease. Grisolle has performed this task in regard to twenty-four cases of pneumonia. While engaged in his researches, he met with eleven cases in which the inflammation was of very limited extent, and attended with mild reaction. For these he prescribed simple repose, rigid abstinence, demulcent drinks, and, in some cases, a laxative,—his humanity and conscience forbidding the trial of this expectant plan in cases of much severity. The patients were admitted, on an average, on the fourth day of the disease; in nine among them the lung was hepatized, in twenty the inflammation was in, and never passed beyond, the first stage. These cases are compared with a series of thirteen others, four only of which had not already reached the stage of hepatization; the general symptoms were mild, the patients young, and they were all bled twice, locally or generally, on an average the fourth day. In order to render the contrast obvious at a glance, we shall place the results obtained in the two series of cases, beside each other:

11 cases of mild pneumonia, not treated by bleeding.	13 cases of mild pneumonia; bleeding performed on an average on the fourth day.
THE CHARACTERISTIC SPUTA.	
Continued till the ninth day.	Ceased before the end of the sixth day, about forty-eight hours after venesection.
PAIN IN THE SIDE.	
Although diminished at the end of four or five days, never disappeared before the eighth, and frequently continued to the twentieth, twenty-fifth and twenty-seventh days. Its mean duration = fifteen days.	Invariably relieved by bleeding, whether this was general or local; disappeared completely on from the second to the twelfth day, and on an average on the eighth day.

FEBRILE REACTION.

Ceased about the tenth day; convalescence commenced the eleventh or twelfth day.	Had ceased abruptly at the end of the seventh day; then commenced convalescence, which was thoroughly established twenty-four hours after.

AUSCULTATORY PHENOMENA.

Commenced to decrease at the end of the second week, that is four or five days after the cessation of the fever, but continued to be more or less manifest till from the twenty-second to the thirtieth day, that is, the lung was still more or less impermeable.	Began to decrease with the cessation of the fever, that is, the seventh day, and in the subjects carefully examined till their departure from the hospital, the lung had completely recovered its permeability, on an average, on the twelfth day.

It seems to us that no one can compare these two series of cases together, without admitting the efficacy of bloodletting in those in which it was employed. Having so often witnessed, in suitable cases, the influence of bleeding in reducing the force and frequency of the pulse, in diminishing the heat of the skin and rendering it moist, removing delirium, calming restlessness, relieving headache, lessening dyspnœa, and removing or greatly moderating the pain, in pneumonia; for us to doubt its utility would be wholly to discredit the evidence of our own senses, At all events, the loss of blood cannot be so deleterious in pneumonia as some modern authorities have represented, as the following list of cases, selected for the purpose of proving this fact, abundantly establishes:

Bleedings few, repeated or large.

	Cases.	Deaths.	One Death in
Wunderlich	47	3	15.16
Ruef	94	5	18.8
Gerhard	26	1	26
Bang	54	2	27
Wossildo	112	4	28
Lacaze	42	1	42
Burkardt	60	1	60
Skoda	64	1	64
J. F. Meigs	18	0	0

But while this list of cases shows that bloodletting in pneumonia is not attended by the fatality represented, in certain quarters, to follow its use, we must state, on the other hand,

that its influence over the disease has been greatly exaggerated, and those who have bled most largely, have not attained better results than others, who have bled much more moderately. Thus, Bouillaud, the most sanguinary physician of modern times, bleeding upon his plan of *coup sur coup*, drew blood from his patients to a mean amount of four pounds five ounces; and yet he did not obtain, in any single point of view, more favorable results than Grisolle, whose patients lost on an average scarcely two pounds and a half; so that the former caused each of his patients to lose a mean quantity of blood, not only without the least benefit, but doubtless to the positive and serious injury of their constitutions. The results attained by Louis, in his investigations on the influence of bloodletting in pneumonia, first led the modern medical mind to a clearer perception, and to juster views, of its real utility. He proved—(1.) that bloodletting has a happy effect on the progress of pneumonia; that it shortens its duration; that this effect, however, is much less than has been commonly believed; but that patients, bled during the four first days, recover, other things being equal, *four or five days sooner than those bled at a later period:* (2.) That pneumonia is never arrested at once by bloodletting, at least, not on the first days of the disease; and if an opposite opinion had been maintained, it was because this disease had been confounded with another; or because, in some rare cases, the general symptoms rapidly diminish after the first bloodletting, but the local symptoms, *crepitation*, &c., for the most part continue to be developed not the less for this evacuation (*a*). An examination of the records at the Massachusetts General Hospital, by Dr. James Jackson, confirmed the results obtained by Louis; and it is worthy of note that, the sagacious Cullen long ago announced that bleeding is more effectual in pneumonia, when practised in the course of the first three days, than afterwards (*b*).

But if moderate and judicious bleeding be demonstrably efficacious in certain cases of pneumonia, it is, on the other hand,

(*a*) Researches on the effects of bloodletting, 48, '9.

(*b*) First Lines of the Practice of Physic, by William Cullen, M. D. (Edinburg, 1805), 134.

equally demonstrable that indiscriminate and frequent bleedings are worse than none at all. It has been shown by Kennedy that, as repeated bleedings diminish the red corpuscles, and not the fibrin, the thinned blood, especially in pneumonia of asthenic type, with its excess of fibrin, has a tendency to exude more readily into the air-cells; and hence the pneumonia, from being single, may become double, or, being confined to one lobe, may diffuse itself over the entire lung. In the form of pneumonia prevalent in this section for a few years past, whether occurring in connection with the tokens of a malarial or periodic fever element, or not, local depletion is the only mode in which it has been usually proper to take blood; and it is admitted by the champions of the non-bleeding plan by venesection (Bennett, Markham, etc.), that, especially when the parietal pleura is involved in pneumonia, the loss of blood by leeches or cups does, beyond doubt, by some reflex action, as they allege, conveyed from the skin to the vasomotor nerves of the inflamed organ, relieve pain and dyspnœa. Every physician of much experience must have seen great benefit ensue from local bloodletting, when there was much soreness or tenderness upon pressure, and especially by leeches, if the tenderness upon pressure were very acute; and it will generally be found, as Graves has remarked, that in all inflammatory affections of the chest, local depletion is indicated in proportion to the pain and tenderness of the chest, complained of by the patient; and little good, comparatively, is obtained by it, when there is no pain and tenderness. Pain is, of itself, a serious feature in pneumonia, as well as in all diseases in which it is a prominent symptom; and its removal never fails to encourage and elevate the spirits of the patient, and is, therefore, positively beneficial. As furthering the same purpose there is no doubt that the application of a poultice or fomentation over the seat of pain, as warm as can be well borne, especially while the leech bites are still bleeding, is of no little utility. Indeed, Biett treated every case of pneumonia that appeared in his wards at St. Louis, during an entire year, by emollient drinks and linseed poultices; and in vindication of his practice, could point, it is said, to a low rate of mortality! If we are not called

until the second stage is completely established, and the patient has been heavily drugged by repeated doses of some quack purgative pills,—shorn of his strength with an increase of disease,—and pain in the side and considerable dyspnœa exist, nothing acts so well as a *large* blister. It is astonishing to witness, in some cases, how completely the patient appears to be relieved of the urgent symptoms just mentioned, while physical exploration fails to indicate any diminution of local disease.

After speaking of the treatment proper during the *exacerbation*, Dr. Manson proceeds—

III. "The administration of medicine is generally postponed until 9 or 10 o'clock, P. M., at which time a full dose of calomel (fifteen or twenty grains) is administered. If diarrhœa, or a tendency thereto is present, eight or ten grains of Dover's powder are combined with the mercurial. Two or three hours thereafter, this is followed by a full dose of quinine (fifteen or twenty grains) in pills, or diffused in a wine-glassful of cold water. Three or four hours are now allowed to elapse; we now usually repeat the quinine in doses of five to ten grains, in proportion as the pulse may have been reduced, until thirty-five or forty grains have been administered, and the patient is then left at rest. According to my experience, the chill cannot recur whilst the patient is under the influence of the above quantity of the alkaloid; the fever may, however, continue, but usually in a very moderate degree. Should this be the case, the local bleeding is repeated in the evening exacerbation, and calomel, in small doses of one or two grains, with one-half to one grain of ipecac., or two grains of Dover's powder, is given every one or two hours. After midnight, the quinine is repeated in the same manner, or in lesser quantity, &c."

1. *The administration of Calomel.* It has doubtless struck the reader as somewhat singular, that, unless when the *chill* requires the aid of the physician,—and Dr. Manson says that, "*generally*," it does not,—or when bleeding, general or local, is practised during the *exacerbation*, Doctor Manson should defer doing anything for the relief of his patient, until 9 or 10 o'clock at night! Why is this? Is not the disease progressing? Can any good reason be assigned for delaying the administration of medicine, in the cases in question, until a particular hour shall have arrived? Is there any special virtue in calomel when given at "*nine or ten o'clock*, P. M."? Shall the patient lie in bed, and impatiently await the arrival of the hour, when the hand of the physician is first interposed for his relief? We

had supposed that, in the case of any grave disease whatever, it was wise to commence the treatment as soon as possible. Doctor Manson, however, thinks otherwise; and in more acute diseases than one, he has a special fondness for commencing the administration of medicines at particular hours. Thus, in remittent fever (*a*), and in typhoid fever (*b*), as well as in pneumonia, medicines are usually first given at particular hours; and, in each case, these hours are pretty much the same. But, as Dr. Manson belongs to the heroic school of practitioners, when he does strike he strikes effectually; and as the medicines prescribed in each of these diseases are nearly the same, so is the result equally fortunate. In each instance, the disease is cut short, or very nearly so. This is, at least, reducing the practice, in these important diseases, to a very simple formula. In almost all diseases attended by much irritability of stomach, the administration of medicines, excepting such as are calculated to allay gastric irritation, should be deferred until the stomach becomes retentive. In respect to calomel, however, even this exception is not generally to be made, since it is well known to possess marked power in relieving gastric irritability, and will be retained when scarcely anything else will.

Nor does it appear less singular, that, in a form of pneumonia in which, *according to Dr. Manson*, the expectoration usually "presents more of the character of a *defibrinzed blood* than as a product of inflammatory action," and in which the "condensation of the lung is *not* due to an inflammatory process with lymph effusion," calomel should be so largely given, when it is well known to be one of the most powerful anti-plastic agents we possess. Nor is the use of calomel, in the doses recommended, consistent with Doctor Manson's statement that, in the disease under notice, the intestinal canal is "generally in a state of *phlogosis* or exalted sensibility," and in which "*hypercatharsis* is often induced by the *mildest aperients*"; for, notwithstanding all that may be said about the employment of calomel, by a

(*a*) Transactions of Seventh Annual Meeting Medical Society of North Carolina, 60.

(*b*) Stethoscope, vol. iii. 137.

large number of physicians, in dysentery, as a remedy opposed to the existence of inflammation wherever seated, still, it cannot be denied, on the other hand, that it has a marked tendency to bring on inflammation of the large intestines, dysenteric straining and mucous and bloody stools, if given in doses of fifteen or twenty grains, and especially if it be long continued, even in much smaller doses. Indeed, it is chiefly in consequence of this effect of mercury, and the marked impressibility of the bowels in the South, that, as Dr. Ames remarks, mercury has been for a great length of time slowly falling out of use among us. An artificial, but exceedingly destructive mercurial muco-enteritis is not unfrequently observed, when calomel has been largely administered; and this occurs much oftener, and with much greater intensity, if it is associated with tartar emetic. There is something peculiarly irritating in the association of these two articles; and, after having directed special attention to the subject, we have become entirely convinced that the calomel contributes most to the deleterious influence in question. We do not desire to be understood as stating that, in the same doses, calomel is more irritating than tartar emetic; but, our position is, that, in such doses as are safe in the large majority of cases of pneumonia in our section, tartar emetic is less apt to develop intestinal inflammation than the smallest doses of calomel recommended by Doctor Manson, if they be continued for three or four days in succession. Thus, a patient will bear $\frac{1}{4}$-gr. of tartar emetic every two hours (and this is the maximum dose with us), if guarded with only two or three drops of laudanum, in a solution of gum arabic, for days in succession, with far less evidence of intestinal irritation, than he will bear one or two grains of calomel, with two grains of Dover's powder, if given at the same intervals, and for the same space of time. We must not be understood, however, as abandoning calomel entirely. We contend only that it is much more safe, and that better results follow its use in smaller doses than Dr. Manson has advised. We rarely give it in pneumonia, in more than one-half the quantities employed by him.

2. *The administration of Quinine.* In pneumonia connected with a malarial or periodic fever element, it was the practice of

physicians, after bleeding during the febrile paroxysm, to give cinchona freely, certainly more than a hundred years ago. Chomel states that it was used in those pneumonias which succeeded to intermittent fevers at Mayence, in 1751, '52 and '54 (*a*). "The older physicians," says Dr. La Roche, "used bark freely in such cases, a point upon which the reader may refresh his memory by referring to the writings of Torti, Morton, Lautter, Alibert, Mongellaz, Gouraud, &c." When Morton detected a malarial element lurking beneath the mask of pneumonia or pleurisy, "he made *bark the principal foundation of his treatment*"; and Lautter, practising at Luxembourg, in 1759, '60, in cases marked by high fever, hard pulse, extremely acute pain in the side, and great dyspnœa, after moderating the violence of the symptoms by bleeding during the height of the exacerbation, (the blood being covered with an inflammatory crust), "struck at the root of the disease" by administering an ounce of bark during the ensuing remission, and continued it until the affection was radically cured (*b*). "There is no one among us," says Sarcone, "who is not in a condition to present numerous observations respecting the happy results obtained by cinchona in affections of the lungs, *combined with periodic fever.* Messrs. Serao, Ventapane, Rubertis, Cinque, Visoni, and others, have effected memorable cures with bark in diseases of the same nature, not only this year (1764), but also in the preceding ones. I myself, though confessedly young in the profession, and of little importance in comparison with men of eminent talents, such as those I have had the honor to name, am able to *adduce numerous examples of the useful employment of that remdy in the diseases in question*" (*c*). Bailly,—whose observations were made in Italy, and principally at Rome, during the years 1820, '21 and '22,—in pneumonia and pleurisy, complicated by intermittent fever, gave cinchona in half-ounce doses to arrest the febrile access, while he combated the local disease by bloodlet-

(*a*) Dic. de Méd , t. xxv. 218.

(*b*) A Treatise on Malignant Intermittents, by J. L. Alibert, 3d edit. (1807), 46-48.

(*c*) La Roche, op. cit., 486.

ting, (the blood drawn presenting the buffy coat); and states that Torti, Morgagni and Morton, have reported cases of intermittent arachnitis, hemiplegia, convulsions and apoplexies, cured by cinchona; and that all of these observers believed that those diseases which prove fatal with apoplectic symptoms, ought to be considered as true apoplexies, if they are not curable by this bark (*d*).

Such having been the practice of many eminent physicians, in the cases under consideration, previous to the discovery of quinine, the sulphate of this alkaloid, as being on every account more eligible, soon superseded the bark in substance. We presume that, at the present time, there are few, if any, physicians to be found anywhere, who, in pneumonia presenting unmistakable evidences of a malarial impression, would hesitate to make the *sulphate of quinine* an essential part of the treatment. In the cases where pneumonia presents itself under an intermittent form, says Chomel, it is proper, *as soon as this characteristic is recognized*, to have recourse to the sulphate of quinine. (Loc. cit.) Grisolle says that, in *intermittent and remittent pneumonia*, the administration of quinine constitutes the *principal indication, that which ought to predominate over all the rest;* that it must be remembered that the alteration of the lungs is in relation with the number and the duration of the febrile exacerbations or of the paroxysms; and that it is to put a stop to, to remove or to render them of less duration, (*c'est donc à les faire cesser, à les éloigner ou à les rendre moins longs*), that all the efforts of the practitioner must be directed. It is necessary, therefore, he continues, to administer, one after another, even during the febrile access, the sulphate of quinine in the dose of twelve, eighteen, twenty or thirty décigrammes, by the mouth, and in clysters (*a*). Valleix says if there be any signs of an intermittent law discoverable, (*la pneumonie*

(*a*) Traité Anatomico-Pathologique des Fièvres Intermittentes, etc., par E. M. Bailly, de Blois, Paris (1825), 265, '6.—This work has been received since the part relating to the pathology of the disease under notice was written.

(*b*) Op. cit., 127.—A *décigramme* is equal to a little less than two grains, French.

intermittente ou rémittente), *we ought not to hesitate to give quinine* (*b*). Prof. Morehead says—

"In the treatment of *febrile* pneumonia, in addition to such amount of local blood-letting and tartar emetic, and use of blisters, as the symptoms may justify, quinine should be given in adequate doses *during the remission.* It may be combined with tartar emetic. From five to eight grains of quinine, with one-sixth to one-fourth of a grain of tartar emetic, given at intervals of two or three hours for five or six doses, will, in general, suffice to check and then stop the febrile recurrences. When this effect on the febrile symptoms has been produced, it will generally be found that improvement in the pneumonia will at once commence; and in a large majority of cases, if the recurrence of the febrile state be prevented for some days, the inflammation of the lung will be speedily removed. That this is a therapeutic fact I am satisfied from the observation of many cases. Indeed, *I am not acquainted with anything more striking and satisfactory in the whole range of rational therapeutics than the progressive but speedy restoration of a hepatized lung, coexisting with fever of a remittent type, when the exacerbations have been controlled by the adequate use of quinine.* It is true that small local detractions of blood, the application of small blisters, and the use of quarter-grain doses of tartar emetic, have been generally had recourse to at the same time; but it is quite impossible for any one familiar with disease, and the action of these means in these degrees, to attribute the benefit chiefly to them, and not to the circumstance of the prevention of the febrile exacerbation by the action of quinine. * * * The same principle of treatment has been also applied to many of the cases of *primary* pneumonia in asthenic subjects. * * I have latterly, in all cases in which the remission was well marked, given quinine in combination with antimony, in the same manner as in the febrile cases, and very frequently with the same good effect; though I think that the improvement in the lung has taken place more slowly. It is also very true, that in some of these cases in which even the remission has been well marked, we have met with disappointment; the quinine has failed to control the exacerbation" (*b*).

It is obvious from these quotations, that, in the disease under consideration, the value of quinine is well known and duly appreciated abroad. Let us now see how the matter stands in our own favored land.

In speaking of *bilious pneumonia*, which, in this country, is generally associated with miasmatic fever, or a miasmatic state of system, Prof. Wood states—

(*a*) Guide du Médecin Praticien, t. i. 464.

(*b*) Researches on Disease in India, vol. ii. 370, '71.—It may be well to remind the reader, that Dr. Morehead uses the term *primary* pneumonia in its generally received sense, and designates by the term *febrile*, the pneumonia which complicates intermittent and remittent fever.

"Whenever the remission is decided, after the thorough evacuation of the bowels, and the abstraction of whatever amount of blood may be deemed advisable, recourse should be had to the *early use of the sulphate of quinine*, as in remittents without the same complication. By arresting the paroxysm, we do infinitely more good, in relation to the inflammation, than the stimulant action of the quinia can do harm. There is no stimulant so powerful as the paroxysm itself, and each recurring one has a strong tendency to sustain the inflammation. Indeed, the pneumonic symptoms are almost always worse in the exacerbation of the fever, and decline as that remits. From twelve to eighteen grains of the sulphate of quinia should be given between the paroxysms, usually in divided doses of one, two or three grains, according to the length of the interval. Should the disease put on, as it not unfrequently does, a typhoid character, this medicine becomes still more proper, and in malignant cases it is *absolutely indispensable.* In these latter cases, *it must be administered unsparingly;* for it is of the utmost importance to prevent the recurrence of the paroxysm. The quinia may often be advantageously combined with calomel, opium and ipecacuanha. Very commonly, the pneumonia subsides along with the fever, and requires little further treatment when the paroxysms have been superseded. Should it remain, however, it may then be managed as if an original affection. Should the disease be intermittent, the propriety of employing this remedy is still more obvious" (*a*).

The doses in which Prof. Wood advises that quinine should be usually given, in cases of ordinary occurrence, are smaller than those in which it is generally prescribed in the South; still, if twelve to eighteen grains are given between the paroxysms, in cases of moderate severity, as he directs; and if, in bad or malignant cases, it be "*administered unsparingly*," as he also directs, the patient will be exposed to small risk of dying from the want of quinine.

In his work on Diseases of the Chest, Swett states—

"When pneumonia occurs during the progress of an intermittent fever, local and sometimes general bloodletting are necessary to mitigate the inflammatory symptoms. But you must remember, in these cases, that QUININE IS THE CHIEF REMEDY, and not allow the existence of a local inflammation to deter you from the use of anti-periodic remedies. For the *first indication is to neutralize the miasmatic influence*, and when that is accomplished, to combat the local inflammation, if it has not already yielded." (P. 113).

The late lamented and eminent Professor Drake, whose great work is said to have no parallel of its kind, in medical literature, —before proceeding to speak of the *methodus mendendi* demanded

(*a*) Prac. Med., vol. ii., 4th edit., 28, '9.

in "*bilious or malarial pneumonitis*," states, that the treatment advised is based on conversations in respect of this disease, with more than one hundred and fifty physicians scattered from the shores of the Gulf of Mexico to those of the great lakes, and upon the experience of others published in the Journals, taken in connection with his own in one of the middle latitudes. After remarking upon bloodletting, tartar emetic, and calomel, he goes on to observe that—

"The *sulphate of quinine* is one of the most important, in many cases the most indispensable, remedies in this disease. By it, we meet the autumnal fever diathesis. When this is well developed, as will be manifested by the low tone of inflammatory action, and by the tendency to, or actual occurrence of, intermissions, the quinine is strongly indicated, and without it the patient may perish. In more inflammatory cases, it is frequently required after a single bloodletting; and in the most inflammatory, demanding two or three bleedings, it is necessary to administer it. It should, in general, be combined with other medicines; and experience has shown, that it works well in conjunction with tartar emetic and opium, and also (which is a common mode) with calomel and opium. As to the quantity, it may rise from ten or twelve to twice that number of grains in the twenty-four hours; and a good method is to give one-fourth the quantity every six hours. In cases of an intermittent and ingravescent character, it may, however, be necessary to administer it in much larger quantities, combined with opium, a short time before the expected paroxysm, as for a malignant intermittent." (P. 872–'73).

In discussing the *sedative* action of the quinia, and the necessity of using it in all febrile diseases, to moderate the action of the heart and arteries, Dr. La Roche says:

"The only instances of pneumonia in which quinia has been found undeniably and decidedly useful for the purposes contended for, are those in which the periodic element is marked by well-defined remissions, or complete intermissions, and the malarial complication is clearly exhibited. It matters not whether these intervals be of long duration, and exhibit the quotidian or tertian types, or whether the exacerbation or paroxysm return after a few hours, or even a single hour of repose—a phenomenon which we know to occur in other forms of disease. In these, and in every other case in which this periodic element is manifest, quinia should, undoubtedly, be resorted to, more especially when the disease displays a pernicious or malignant tendency—a circumstance which renders the recurrence of a paroxysm of the utmost danger. These cases, in which the inflammation is in all probability modified by the malarial taint, in such a way as to tolerate the use of remedies, which, under other circumstances, could not be borne with impunity, *resist every other mode of treatment.* They cannot be cured by anti-phlogistics alone; for although by these the inflammatory affection

of the lungs may, if uncombined, be removed, the malarial fever or taint is not to be so destroyed; and the recurrence of every paroxysm or exacerbation has the effect of aggravating the local disease; which cannot, therefore, be eradicated, unless a stop be put, by anti-periodic remedies, to the complicating complaint. With the cessation of the latter, the pneumonic inflammation, if it has not reached the first stage, generally disappears also. In other cases, it abates considerably, as indicated by an improvement in the general symptoms and physical signs. In others, again, it continues. When this occurs, it must be treated by the usual means. * * * * The use of quinia in the same disease is *universally resorted to. I have myself employed it largely by the mouth or rectum, and have seen it employed by others both at home, in the south of this country, and in Italy, where instances of the kind are not uncommon.* Of course, it is a matter of indifference whether the inflammation be in the lungs or elsewhere. *Whenever it assumes a decidedly remittent or an intermittent type, and is blended with a malarial fever, or modified by a malarial taint, and more particularly whenever signs of malignancy or congestion manifest themselves,* QUINIA SHOULD BE RESORTED TO, AND THAT PROMPTLY. We must, as is well remarked by Dr. Merrill, *take advantage of the* FIRST REMISSION, otherwise we may expect the second exacerbation to become more violent than the first; an effect due, in part, to the fact that every violent paroxysm of fever—more particularly when preceded by a chill—tends to aggravate the local inflammation or congestion, and prostrate the nervous power." (P. 484–'7).

We have not space for comment upon these extracts from eminent American authors; nor is it necessary.

If we should now turn to the medical Journals, we shall find abundant evidence that the use of quinine, in pneumonia imbued by a malarious influence, is well understood by our physicians; and from the much more frequent occurrence of instances of this character in regions farther South, than in the section in which we reside, quinine has come to be employed there, in the vast majority of cases of pneumonia, as well as in other phlegmasiæ, of various grades and types. Among those physicians of the South, who, in respect of the subject in hand, have contributed to the current medical literature of the times, no one deserves a higher meed of praise than the late Dr. Boling, of Alabama. In an able paper (published in July, 1844), "*On the Treatment of the Inflammatory Affections of Malarious Districts,*" he has detailed cases of meningitis, convulsions (occurring in connection with remittent fever), bronchitis, pneumonia, dysentery, metritis, cystitis, acute rheumatism, etc., presenting tokens of the element

of periodic fever, in which *quinine* was promptly administered; and he justly insists upon its forming an indispensable part of the treatment, in all cases of the character under consideration (*a*). In a subsequent paper, in which he reported cases of pneumonia, etc., to show that the *pulse may be reduced in frequency below the normal standard by quinine*, he states, in reference to these cases, that he has "no hesitation in saying that they are of daily occurrence in the hands of *hundreds of Southern practitioners*"; and, "should any Southern practitioner be heard to say that he had never observed such an effect from quinine, there are many whom it would be difficult to convince, that he appreciated its advantages, or could avail himself of its benefits to the full extent in the treatment of our febrile and inflammatory diseases" (*b*). For the purpose of reducing the pulse to something like the healthy standard, in the treatment of the phlegmasiæ, he generally gives the smallest doses capable of producing this effect,—the quantity being regulated by the effect produced; and when six or eight grains every third or fourth hour fail, he prescribes doses of ten or twelve grains. Dr. Ames, in speaking of pneumonia of the same latitude, (Montgomery, Ala.), says that *quinine* constitutes a very important part of the treatment in that climate; that if sometimes inefficient, it is also at times indispensable; and that, in malarious districts, it is a safe rule to begin the treatment of pneumonia by making quinine a component part of it. His own custom is, to give, usually every fourth hour, six grains of quinine, one-sixth grain of morphine, and two drops of a saturated radical tincture of aconite, until the peculiar effects of quinine on the head become very well marked; and if the signs of amendment are not then satisfactory, or such as may be properly attributed to the quinine, to discontinue it, and give the aconite in water (*c*). Dr. Anderson states that the intermittent or remittent fever, accompanying the pneumonia of

(*a*) American Journal Medical Science, N. S., vol. viii. 87.

(*b*) New Orleans Med. and Surg. Journal, vol. ix. 749.

(*c*) New Orleans Med. and Surg. Journal, vol. x. 437.—It may be proper to state also, that two hours before giving each dose of quinine, a teaspoonful of a dilute solution of phosphorus is ordered by Dr. Ames.

South Alabama, is more dangerous than the pulmonary inflammation,—the signs of periodicity, however, being often obscure; and that *quinine* should constitute "the *chief* remedy": others being used "merely as adjuvants" (*a*). In a letter to the writer, Prof. A. P. Merrill, of Memphis, states, that his attention was called to the importance of giving cinchona in the *bilious pneumiona* which prevailed at Natchez, Miss., where he then resided, as early as the winter of 1824; that when quinine came to be used extensively in the treatment of periodic fevers, it was not uncommon to use it in pneumonia; his impression, however, is that, in general, its exhibition was not considered judicious until after the inflammatory symptoms were considered so far subdued as to admit of pretty free secretion and expectoration; "but that *now it is common, among physicians of all this region of country, to use quinine in the early stage of pneumonia*"; and that he has "good reason to believe that the *quinine practice is pursued extensively in Tennessee, North Mississippi and Arkansas.*" Dr. Goodridge A. Wilson, now of Granville county, North Carolina, whose observations were made at Clarksville, Virginia — situated immediately on the Roanoke river,— and in the surrounding country, is a highly useful practical paper on the "*Influence of Epidemics on Sporadic Diseases*," has given some striking illustrations of various affections, and among them *pneumonia*, complicated by the phenomena of periodic fever, in which he judiciously prescribed quinine, in connection with other appropriate remedies, and affirms—"*As early as the year* 1838, I had to unlearn all that had been taught me of this agent as a stimulant and tonic, and of the dangers attending its administration in inflammatory states of the system. I have often noticed its effects in that class of cases complicated by cerebral determinations, and can safely say I have never known injurious consequences to follow. If stimulant at all, it has not acted in my hands as stimulants are wont to do. In many cases of high nervous excitability, it has had soothing and *sedative* effects" (*b*).

(*a*) N. O. Med. and Surg. Journal, vol. xi. 78. (*b*) Stethoscope, vol. ii. 256.

Were it necessary, we might greatly extend the list of authorities to prove, that, in pneumonia presenting tokens of a malarial or periodic fever impress, judicious practitioners, every where, regard the employment of quinine, if not the *sine qua non*, at least the most important, and the most precious agent known to Science. And he must be a great sluggard, who would hesitate, under such circumstances, to prescribe it according to the exigencies of the case. Every physician of our acquaintance, who has seen practice enough to have formed any opinions at all worthy of regard, uses quinine in such cases, and esteems it as of the greatest value. Indeed, within the circle of our professional intercourse with medical men, so common has it become to use quinine in nearly every case of grave character, even in many instances when there are no trustworthy indications of a malarial taint, that we fear there is some danger of our employing it quite as a matter of course and familiar routine, in every case that we may meet, very much after the manner of using calomel some quarter of a century ago. We cannot concur with any one who thinks that quinine is left out of the treatment in any case whatever, occurring in this region, if there be any good reason to suspect the existence of a malarious complication. It is very true, that for a long time, very erroneous ideas were generally entertained in respect to the use of quinine in fevers and inflammations, under the belief that if either of these states were well marked, it was contra-indicated, and calculated to do no inconsiderable mischief; but these therapeutical dicta were already rapidly passing away when we first entered upon our medical studies, and we were not educated under the thraldom of such opinions. In the first work on the Practice of Medicine that we read, the doctrine is taught that quinine is a *sedative;* and that when we are desirous of making an impression at all decided, in allaying the febrile disturbance of the functions generally, five grains, repeated at intervals of two hours, is the smallest dose which we should think of prescribing for an adult. And in that work it may be seen that Bailly himself took a hundred grains of the sulphate of quinine in five days, for a slight febrile disorder, when he was in Rome (in 1822), without being

able to detect any irritation, after an attentive analysis of his own feelings, which this quantity of medicine, says he, would certainly have produced, if it had been of an irritating or stimulating nature; that Dr. J. K. Mitchell had given sixteen grains of quinine in remittent fever, "and with such good effect that, from that time, the violence of the disease was subdued and convalescence soon established;" that Dr. J. E. May had taken, in a severe attack of remittent fever, commencing in the exacerbation, "fifteen grains of quinine, which he repeated every two hours until a drachm had been thus used"; and that his "recovery was rapid"; that in "another case of fever of a malignant character, of three weeks' duration", in which, with many bad symptoms, the patient had "parched lips, an utter inability to sleep, which had lasted for several days, with delirium during the night," Dr. May had given *forty grains* of quinine "in two hours," with immediate improvement in all the symptoms, and uninterrupted advance to recovery; that Dr. Drake had been accustomed, for many years, to give quinine in doses of ten or fifteen grains, and that he had known a man take, by mistake, a drachm at one portion, with no other inconvenience than a roaring in his ears (*a*). And when we entered Jefferson Medical College, in the autumn of 1842, we heard the eloquent and lamented Professor J. K. Mitchell recommending quinine in remittent fever, as "*the* remedy,—to be given in the earliest remission"; and stating that under its influence (we quote from our *notes*, taken at the time), "the pulse falls, headache abates, and the patient expresses himself as getting better." He used to dwell with especial emphasis upon the now well-established fact, that this remedy was not to be rejected, in miasmatic diseases, in consequence of the existence of local inflammations. When we repaired to the hospital of Baltimore City and County Almshouse, in 1843, as one of the Resident physicians, we there saw quinine given in almost every possible variety of malarial fever, without any sort of regard to the intensity of the fever, if the symptoms were urgent, and in doses varying from ten to thirty grains, *pro re*

(*a*) Stokes and Bell's Practice, second American edit. (1840), 596–'9.

nata. It was the established practice of the hospital, and had been for three years.

Such were the ideas we had imbibed, in regard to the use of quinine, when we entered upon our professional career in this vicinity, in 1844.

In surveying the ground over which we have just passed, it is evident, from the multiplied evidences presented, that, in so far as *the use of quinine in pneumonia offering reliable indications of a malarial complication* is concerned, "we of the South" have *not* lingered behind in the rear of advanced therapeutics, nor, on this score, exposed ourselves to the sweeping charges of incompetency and malpractice, preferred against us by Dr. Manson; and if any "neophyte" should not perceive the "signs" that constantly loom up before him, as beacon lights to illumine "the labyrinth" he will be called on to traverse, he can certainly have no one to censure but himself, since he prefers darkness rather than light, only because he is too lazy to open his eyes.

In regard to Doctor Manson's special method of administering quinine in "malarial pneumonia," we have only to say that every physician has some little fancy to exhibit in this matter; and so long as the clear and rational indication for its employment is borne in mind, this is of little moment, if it be promptly given, and in sufficient quantities to make a decided impression. Usually, we prescribe from sixteen to twenty-four grains during the first remission, in two doses, four hours apart,—commencing early enough to elicit the physiological effects of the remedy before the anticipated exacerbation; and the following is a favorite formula:

℞—Quin. sulph. ...gr. xvi.–xxiv.;
Acid. sulph., a. gtt. xxiv.–xxxvi.;
Liquor Doveri, gtt. xxx.;
Sp. æther. nit., ʒ ij.;
Aq. fluv. ..... ʒ xiv.—M.
Dose, one-half at once, the remainder in four hours.

In respect of the quantity that may be necessary, we concur with Doctor Manson that, in this latitude, the patient is entirely safe from the danger of a paroxysm after thirty-five or forty

grains have been administered, and very generally twenty grains will suffice for this purpose; but it is best to continue the quinine from day to day, so long as the fever persists. It is a good rule to give the quinine during the remission, and other agents that are indicated, during the exacerbation of the fever. If the patient be seen soon after the onset, the disease may sometimes be cut short, by sedative doses of quinine, in the first stage; that is, the inflammation scarcely passes into the second stage. If, however, the second stage be well established, when the case first comes under care, the results are not so decided; still, quinine is of almost indispensable importance in this stage, and, by physical exploration, we can often trace, from day to day, the power of this agent in promoting the removal of the intra-parenchymatous exudation. And in certain cases of pneumonia of asthenic type (especially prevalent during the past few years), we have often given quinine with great benefit, when no reliable evidences of a malarial element could be detected, upon the most rigid analysis of all the vital phenomena presented. In cases of this character, we have been in the habit of giving, after the bowels are opened, half a grain of calomel, from one half a grain to a grain, each, of ipecac., and golden sulphur of antimony, with from one-eighth to one-fourth grain of opium, and from two to four grains of quinine, every two or three hours; and if the skin had been dry, coolish, and husky, with soft compressible pulse, and depressed countenance, in from twenty-four to thirty-six hours, we have often seen the skin become warm and perspiring, the pulse fuller and stronger, sometimes less frequent, and the facial expression brighter and more hopeful. In the cases in question, the combination, just mentioned, has appeared to us to possess diaphoretic properties of considerable potency. In some cases, we have given quinine in connection with senega, which, in pulmonary affections, we are convinced is an agent of no small value.* We cannot admit that, in these instances, the

* Professor Wm. Procter, jr., of Philadelphia, has prepared for us, for some years, a solid extract of senega, which is very convenient to a village and country physician. Five grains of the extract is equal to an ounce of the common decoction of senega, and may be given in pill, or rubbed down in any desirable fluid,

periodic fever element must have tainted the system, merely because quinine was of unmistakable service. To assume the presence of this element, in the total absence of any other proof of its existence, is simply to beg the question. Others, looking at the case from a different stand-point, have reached the same conclusion. Thus, Dr. Corrigan, of Dublin, has found quinine of great value in a form of asthenic pneumonia, quite prevalent in that city during the past few years, under circumstances that preclude the complication of a periodic or malarial diathesis; and, were it necessary, we might array a vast amount of evidence to the same purport.

Having said thus much upon the use of quinine in pneumonia impressed by the poison of malaria, and avowed our conviction of its great importance in such cases; and even in certain cases of pneumonia, of a more or less asthenic type, which present no tokens whatever of the element of periodic fever; we will not be accused of a disposition to depreciate its value. But, at the same time, we desire to state emphatically, that, in our judgment, quinine is often pushed entirely too far in cases in which it is obviously indicated, and, in many others, and much the larger number, in which there exists no rational indication for its employment at all. We have no doubt that, in relying too exclusively upon its powers, and, consequently, neglecting other remedies that ought to have been administered, some cases of pneumonia have perished that ought to have recovered. Indeed, such cases have come within our own knowledge. Nay, more: we have often given quinine in pneumonia, unmistakably connected with the element of periodic fever, without any perceptible influence over the local inflammation. The miasmatic element has been eliminated, so to speak, but the pneumonia has progressed, and required the use of appropriate remedies, as in the ordinary uncomplicated disease.

Dr. Manson states that when "malarial pneumonia" does not yield to quinine, and other remedies advised by him, he has

as camphor water, liquor ammon. acet., &c., with the addition of sugar and ext. of liquorice. Tilden & Co. make a fluid extract of senega, which is, also, a very convenient preparation.

ventured on *small doses* of tartar emetic, in unison with the quinine: and that, for a knowledge of this combination, he is indebted to Dr. Blakiston. Quinine and tartar emetic had been used in connection in this country, long before the publication of Dr. Blakiston's treatise. We allude to this combination of remedies, however, under the circumstances specified, for the purpose of commending it; and for the further purpose of expressing our conviction that, in pneumonia unconnected with the poison of malaria, and when the state of the vital forces do not clearly contra-indicate it, *tartar emetic* exerts a more potent influence over the inflammation, than any other agent known to us. For a long time, it was habitually used in doses much too high, and when associated with calomel, as previously stated, the combination, although powerfully remedial as regards the pulmonary inflammation, has not unfrequently produced, especially in the South, an uncontrollable gastro-enteritis. During the past ten years, we have never exceeded one-fourth grain of tartar emetic, in a solution of gum arabic in mint water, every two hours, in the most violent cases of pneumonia; and if it be commenced with in doses of one-eighth grain, and gradually increased, giving, at the same time, from twenty-five to fifty drops of laudanum in the twenty-four hours, there are few persons who will not bear it, in cases fitted for its use. In cases deeply poisoned by malaria, and whose nervous systems have been long depressed by the action of this poison, tartar emetic, in *high doses*, as our friend, Dr. Buckler, has remarked, is "little less than murderous" (*a*). So far as we have observed, however, a large majority of the cases of pneumonia, occurring in this section, present no recognizable tokens of the poison of periodic fever. In this respect, as well as in so many others, our experience differs very widely from that of Doctor Manson, whose experience, he states, is just the reverse. The fact is, that, so far as we can learn, every form and grade of malarious diseases are much less frequent, throughout the South, than they were some years ago,—having been supplanted, to a great extent, by

(*a*) Fibro-Bronchitis and Rheumatic Pneumonia, by Thos. H. Buckler, M.D., 132.

typhoid fever, coincidentally with the prevalence of which, other affections, as already stated, have presented a more or less asthenic type.

Dr. Manson is not very explicit in stating the amount of success he has attained in treating "malarial pneumonia" by the plan he so highly lauds. He contents himself with the statement, "that, under proper management, this disease, although of decidedly fatal tendencies, is more under the control of remedial agents than any of the so-called phlegmasiæ"; and that "it is rare that cases thus treated in the *early* stages, do not exhibit the most flattering amendment on the day succeeding the first night on which this plan is adopted, and still more rare, that every dangerous symptom has not disappeared on the succeeding day." But there is nothing very definite in the statement that the disease is more easily cured than any other inflammations; and how many cases do we encounter in the *early* stage? In estimating the value of any method of treatment in pneumonia, there are certain things to be considered, which, of themselves, go far to insure success, or to swell the list of deaths. Let us consider—

1. The influence of *age*. "In new-born infants," says Walshe, "and after the age of seventy, the disease is almost always fatal; between the ages of six and twelve, death is the rare exception; between the sixteenth and twentieth years, scarcely one in fourteen of those attacked perishes; while in each succeeding decade up to the seventieth year, the deaths range between one-fifth and one-seventh of those seized." It follows from this, that the greater the number of persons advanced in life, the greater will be the number of deaths, and the greater the number of young persons, exclusive of neonati, the smaller will be the number of deaths. Of sixty-seven cases between the years of fifteen and twenty, treated by Chomel, only two died; and in both of these a *double* pneumonia supervened, whilst the patients were very much debilitated by previous attacks; one had suffered from an attack of scarlet fever, and the other from typhoid fever.

2. *Extent of the disease* exercises great influence on its mortality. The proportion of deaths is from two to six times greater

in cases of *double*, than single pneumonia. Chomel states that from 1832 to 1839, *one-half* the cases of double pneumonia at *Hotel Dieu* terminated fatally: while in lateral (single) pneumonia, the mortality was about one in seven; and that when the whole of one lung is attacked, *four-fifths* die.

3. *Sex and strength of constitution.* Pneumonia is less fatal in males than in females. During eleven years, Chomel found the mortality to be more than one-sixth of the men, and less than one-fourth of the women; and Grisolle ascertained that, during three successive years, the proportion of deaths among females was nearly twice as great as among the males. The greater mortality among females, is probably due to their greater delicacy of constitution. Thus, *two-thirds* of the deaths in the practice of Briquet were among the feeble, although *two-thirds* of his patients were comparatively strong; and while the mortality among robust Europeans, at the Bombay hospital, amounted to one in eleven, the mortality among the fragile natives amounted to 1 in 2·6. In drunkards, as is well known, pneumonia is particularly fatal.

4. *Complications.* In certain states of the system, and when pneumonia is complicated by other diseases, it is much more fatal. "I have for the most part found," says Professor C. D. Meigs, "that a patient attacked with inflammation of the lungs is not very easily cured, if in an advanced stage of pregnancy, until after her delivery shall have taken place." Of fifteen cases of pneumonia occurring in pregnant women, observed by Grisolle, eleven died; and of thirty-four cases reported by Barclay, at St. George's hospital, twenty presented complications with other diseases, of which eight perished, while of fourteen uncomplicated cases, only two succumbed.

4. *Side affected.* There is reason to think that pneumonia of the *right* lung, is more fatal than that of the *left*. Thus, of eighty-one cases treated, by Chomel, from March 15th, 1842, to March 10th, 1843, in pneumonia of *right* lung, there was a mortality of more than one-sixth, while in that of the *left* lung, there was a mortality of one-fifteenth. And during the eleven years preceding, pneumonia of the *right* lung proved fatal in

more than one-sixth of the cases, while that of the *left* lung proved fatal in more than one-eleventh. It is thought by some, (Andral, Grisolle, Watson), that pneumonia of the *upper* lobe is more perilous than that of the *lower*. Others, however, think differently. Chomel found the mortality of pneumonia of the *summit* to be one-sixth, of the *base* to be one-seventh of the cases.

6. The *prevailing type* of the disease. It has been well ascertained that pneumonia is much more fatal in some years than in others. Thus, it is stated by Kennedy, that in St. Mary's Hospital, 1 died in 8·6 cases in 1854, 1 in 2·15 in 1855; in the Edinburgh Infirmary, 1 died in 2·4 in 1847, '48, 1 in 5·2 in 1848, '49; in the Aberdeen Infirmary, 1 died in 2·7 in 1853, 1 in 10 in 1855, and not one of eighteen cases in 1856; and in the Vienna "*Krankenhause*," 1 died in 7·3 among the male cases, 1 in 3 among the females, in 1851, while 1 died in 3·6 among the males, 1 in 2·6 among the females, in 1855. The treatment was conducted, usually at adjoining periods, by the same physicians. The discrepancy in the proportion of deaths in the same institution, in adjacent years, is remarkable.

7. The *time at which treatment is commenced* exercises great influence over the result. Thus the mortality among Grisolle's patients admitted during the first three days was one in thirteen, while of those admitted after the sixth day, from one-third to one-half died. This shows the importance of early treatment, and of the sick being placed under favorable hygienic conditions, during the first days of the attack.

8. Lastly, *secondary* pneumonia, that is, pneumonia occurring soon after other diseases, as measles, scarlet fever, small-pox, &c., is much more fatal than primitive pneumonia, occurring in the midst of perfect health.

These are the chief considerations, irrespective of any special modes of treatment, which exert great influence over the mortality of pneumonia. It is obvious that if they be not borne in mind, in comparing the value of any proposed plans of treatment, the results attained may be but the expressions of false estimates.

Finally, it has been shown beyond doubt, by the different

methods of treatment adopted by different observers, that, heretofore, pneumonia has been treated by far too actively; and many patients, doubtless, have been literally *doctored* to death. Thus, wonderful results have been published, by several German physicians, showing that, by inhaling occasionally a few whiffs of the vapor of chloroform, and observing the usual hygienic measures in acute diseases, a very trifling per centage of mortality is attainable. Doubting whether the inhalation of chloroform did anything in the way of curing the disease, Prof. Metcalfe, of the New York Hospital, treated twelve cases of pneumonia by simple diet, repose, change of posture, and the use of emetics, when they appeared indicated to remove accumulated bronchial mucus. Of these, only *two* died. One was moribund when first seen; and the other completely spanæmic (the result of Panama fever), and complicated by universal bronchitis. So that of the ten cases in which the expectant plan had a fair chance, not one died. Unfortunately, the age, sex, &c., of the cases, are not given. They show, however, that there is not the *absolute necessity* for resorting to extremely heroic antiphlogistic plans of treatment, which, in the minds of some, appear to be inseparable between an attack of pneumonia and the return to health.

But we must bring this protracted paper to a close. The first draught, as presented to the Medical Society of the State, was very hastily written. In preparing it for the pages of this Journal, at the request of the Committee of Publication, to whom it was referred by the Society, the subject has spread out under our pen to its present extended length. The whole of it has been written by snatches, under the pressure of arduous professional engagements, and mostly at night when enervated by fatigue, and suffering from impaired health. When we commenced it, we had no idea that, in accordance with the plan which we had marked out, it would have expanded to its present dimensions. Such as the paper is, however, we lay it before the profession, to be submitted to a candid and impartial criticism. We have reviewed Dr. Manson's Essay with great freedom, but with not less candor than respect for his undoubted talents. We are not unacquainted with his history; and it

gives us pleasure to state that, as a zealous student and faithful minister in the discharge of his professional duties to the sick, he is a model well worthy the imitation of young physicians, who are too prone to dwarf the vigor of youthful promise, by diverting their minds to other channels, instead of earnestly prosecuting their early studies, and becoming, year by year, wiser men and better practitioners. We thought, however, and still think, that Dr. Manson has emitted many grave errors in the Essay under review, and we have therefore, canvassed that Essay frankly and freely, in the spirit that even "*Homer should be waked up when he nods.*"

If a parting word to the rising race of medical men in the South may be pardoned, we would urge them to beware how they reject the teachings of the best men in the profession, both at home and abroad; and when the young and inexperienced practitioner approaches the bed-side of the sick, let him forever bear in mind the wise injunction of Chomel,—NOT TO TREAT DISEASES, BUT TO TREAT PATIENTS AFFECTED WITH DISEASE. The truths of Science are the same everywhere; and although, as in epidemics, certain differences may exist in certain diseases in different places, and in the same places at different times, yet they are readily appreciated by the well-educated physician, who modifies his therapeutics accordingly. Pathological dicta based upon clinical inferences, and therapeutical deductions based upon insufficient or inaccurate data, cannot carry conviction to the minds of men at this day. Well-observed *facts*, not mere opinions, arrest the modern medical mind, and elicit the homage due to Truth.*

DR. SAMUEL A. CARTWRIGHT'S "*INTERESTING LETTER*" TO DR. O. F. MANSON.

It is well known to the profession in North Carolina, that Doctor Manson is engaged in preparing a reply to the foregoing

* We must here return our thanks to our kind friend, Doctor J. T. Chunn, of Baltimore, for having furnished us a translation of Grisolle's account of *Intermittment and Remittent Pneumonia* (see April number of this Journal, p. 430); and also, for a translation of Chomel's views respecting the crepitant rhonchus (see June number, 532). As accuracy was required, we preferred not to trust to our imperfect knowledge of the French tongue.

Review of his Essay on "Malarial Pneumonia." Indeed, it appears from the *Minutes* of the Tenth Annual Meeting of the Medical Society of the State, held at Statesville, in May last, and while our Review was still passing through the pages of this Journal, and before scarcely one-fourth of it had been published, that "*Dr. Manson read a synopsis of his replication to Doctor Howard's Review of his Essay on Malarial Pneumonia*"! And again, in the October number of this Journal, containing a continuation of our Review, a very flattering notice of the Essay in question, and of other productions of Doctor Manson, is spread before the profession in the shape of *a letter to Doctor Manson*, from Dr. Samuel A. Cartwright, of New Orleans. As we have no disposition to continue the discussion of the subject at issue between Dr. Manson and ourself, and have no idea of replying to his rejoinder to us, unless he should render it necessary, in our judgment, to the cause that we advocate, for us to do so; and as no one can be so purblind as not to see that the publication of that letter, at the time and place mentioned, was intended to forestall or present an offset to our views of Doctor Manson's Essay, we beg to offer here a few observations upon that singular communication.

It appears from Dr. Cartwright's letter *to Dr. Manson*, that Dr. Manson had previously written Doctor Cartwright a very flattering letter on his performances, and that Dr. Cartwright, in return, has not only reciprocated the compliment, but thrown Dr. Manson completely into the background. Doctor Manson, not to be outdone in the generous ardor of grateful eulogy and admiration, after making antiquarian researches among "the neglected and forgotten" tokens of Doctor Cartwright's genius, gallantly returns to the pleasing encounter of encomium, and brandishing aloft "*two gold medals*," "*a silver pitcher*," "*a magnificent gold vase, valued at above* $1000," *and* "*other prizes, &c.*," proclaims aloud,—"*The great medical fact of the nineteenth century.*" So that, so far as the verbal felicities of high-wrought eulogium and mutual admiration are concerned, it must be admitted, on all hands, that Dr. Cartwright and Doctor Manson have balanced accounts. We should have

thought that Dr. Manson had outstripped Dr. Cartwright, had not Dr. Manson thought that, in order to give weight to Doctor Cartwright's praises of his productions, it was necessary for him to play the part of *accoucheur* to Dr. Cartwright's reputation *in North Carolina.* But all badinage aside, let us look at this letter, and briefly examine a portion of its contents.

I. *Dr. Cartwright's opinion that Dr. Manson's essays "ought to be extensively published, and that no medical student should receive the degree of Doctor of Medicine in any of our Medical Schools who cannot stand a rigid examination upon them."*

So far as Dr. Cartwright's opinion of Doctor Manson's Essay on "*Malarial Pneumonia*" is concerned, we have simply to say that, whatever Doctor Cartwright's reputation may be, it cannot canonize error. The essay in question must stand or fall, according to its merits or demerits; and if it enunciates false doctrines, Dr. Cartwright's opinion of them cannot make them right. Unfortunately, in Medicine, as in so many other branches of learning, it is easy to cite authorities on the adverse sides of almost every possible question; and, under the circumstances, we may be pardoned for stating that, if we thought an example of at least questionable propriety worthy of imitation, or if we admired such a mode of logic, we might consent to the publication of letters, touching our Review of Dr. Manson's Essay, that have been voluntarily written us by men of the first distinction in the country, which, at all events, would be deemed as a complete set-off to the one under consideration. We rely, however, upon no such collateral support; and as highly as we appreciate these unsolicited evidences of the coincidence of scientific men in the views we have expressed, we should be sorry to feel the need of bolstering up any production of our own, by permitting a publication of their letters. We have examined the doctrines enunciated in Doctor Manson's Essay, from the beginning to the end; we have taken them up, one after another, and followed them, step by step, throughout his argument. If we have not answered him, let him show it.

II. Dr. Cartwright alleges that—

"*Our negro population are unknown in Europe, and are represented in medical books, as they are erroneously supposed to be, as white men with black skins. Our diseases are almost entirely unknown as our negroes are. They are imagined to be some variety of types observed in the North, instead of having direct types of their own. I am glad that North Carolina, so slow to move, is waking up to the fact that our medical journals, even the old ones, containing the experience of our Southern diseases, are of more value than what the auscultators in the London and Paris* HEAR *unless they had our diseases to listen to. * * * The high price of negroes is making a salutary reform in Medicine. In the Southern States it has already driven the servile submissionists to European medical authority out of the country upon the cities. They cannot succeed in the country, because they cannot cure pneumonia and remittent fevers. In the cities they can make a living by doing a little surgery, editing medical journals, republishing European books with stupid notes and commentaries, and getting professorships in* EUROPEAN *Medical Schools,* LOCATED *in* AMERICA. *The great majority of our city practitioners are either learned servile submissionists to European medical authority, or arrant quacks, either of whom would starve in the country, because they cannot cure the diseases of negroes.*"

1. In relation to this aspect of the negro question, we have not space to say much. In contrast with Doctor Cartwright's allegations respecting the essential characteristics of the negro race, however, we will simply place the opinion of the great Southern physiologist, Doctor Bennet Dowler, "that almost nothing indisputably *reliable* has been published concerning the anatomical, physiological and pathological peculiarities of a population so extensive." And in regard to Dr. Cartwright's peculiar doctrines about Southern diseases, he states himself, in effect, that they are ignored in our Southern Medical Schools, the eminent Professors in which are certainly as competent judges, in the premises, as "the distinguished veteran." But so far as the subject in hand is concerned, we can adduce an authority from which there can be, in this discussion, no appeal,—the authority of even *Dr. Samuel A. Cartwright*,—that, in *pneumonia biliosa* ("the Southern form of pneumonia," as Dr. Manson calls it), we witness the same pathological changes in the lungs that are observed everywhere else. We shall here present it; and in respect of the subject under consideration, we beg to commend it to Doctor Manson as furnishing a complete answer both to Dr. Cartwright and himself. Doctor Cartwright says, in his Essay on *Pneumonia Biliosa*,—

"I have examined the bodies of several persons who have died of bilious pneumony. The lungs were always found diseased. On cutting into them and squeezing them, large quantities of a brown or variously colored corrupted matter will generally be found to issue out. More than a pint of such matter oozed out of the lungs of one subject whom I examined, yet there was no abscess in either lung, or any particular reservoir of matter, but it seemed to be generally diffused throughout its substance. A greater or less portion of the lungs were nearly always found *hepatized*, or changed into a substance resembling liver. I have mostly found the left lung more diseased than the right. Drs. McPheeters, Gustine, and myself, examined a patient who died of pneumonia biliosa, in whom the left lung was remarkably diseased, part of it being *hepatized*, and the other parts abounding with a brown purulent matter; the right was but little affected. Yet what was remarkable, the disease of this patient resembled somewhat an acute hepatic inflammation. The liver was but little affected. The heart, as is not uncommon in bilious pneumony, contained in it substances which seemed as if formed of coagulable lymph, being from one to three inches long and about half an inch in diameter. These substances differed from those which I have observed in the hearts of yellow fever subjects. * * The pleura is generally found inflamed in pneumonia biliosa, and bands of coagulable lymph are often seen extending between the pleura costalis and pulmonalis. The diaphragm is often found inflamed." *

Now, this is Doctor Cartwright's account of the "*morbid anatomy*" of the intra-thoracic organs in *pneumonia biliosa;* and we would ask Doctor Manson whether the pathological condition of the lungs described by Dr. Cartwright "is *widely different* from that described by the majority of authors" in simple pneumonia; and whether it "partakes more of the *character of congestion* than active inflammation," as Doctor Manson alleges is the case in *his* "malarial pneumonia"? It is impossible that Dr. Cartwright can differ more widely from any European author in regard to any medical doctrine whatever, than he does with Doctor Manson in regard to the pathological anatomy of the disease both of them profess to describe. Dr. Cartwright, however, has greatly the advantage of Doctor

* The Medical Recorder, vol. x. 81, '2.—It is fair to add, that Dr. Cartwright states, also, that "there are but few cases in which the mucous coat of the stomach, or some portion of the mucous surface of the intestines, is not inflamed." The latter clause of this sentence seems to support the pathological views of Dr. Manson in respect of the state of the *intestinal canal*, in his "malarial pneumonia." But when we bear in mind the heroic quantities of tartar emetic, calomel, aloes and scammony, with which Dr. Cartwright dosed his patients, we need not wonder that the mucous coat of the stomach and intestines should be inflamed.

Manson; for Dr. Cartwright did make some *post mortem* examinations of "persons who died of bilious pneumony," and it does not appear that Dr. Manson has made any.

We claim to have proved, then, from under Dr. Cartwright's own hand, that Dr. Manson has greatly erred in respect to the fundamental doctrine of his essay; and Dr. Cartwright himself must retract his own account of the morbid anatomy of the so-called "Southern form of pneumonia," or he must retract his strictures upon those of his Southern *confrères*, who maintain that pneumonia does the same mischief to the lungs in the South, that it does to the lungs in Europe, and elsewhere.

2. Nor in respect of therapeutics, do Doctor Cartwright and Dr. Manson agree much better. Dr. Cartwright uses tartar emetic in *enormous doses* (never giving *less* than *three grains*), "as a STIMULANT, in the cold, torpid and insensible cases of pneumonia," to "*produce excitement*" and bring on general reaction; Dr. Manson says that tartar emetic "has paralyzed the already feebly-resisting vital force of its myriad victims," and uses it in only *small doses* (one-tenth or one-twelfth of a grain), to *reduce* "*arterial reaction*"; Doctor Cartwright uses *quinine* mainly to "prepare the system to be properly acted on by other agents, particularly *purgatives*"; Doctor Manson uses *purgatives* to prepare the system to be properly acted on by *quinine;* Doctor Cartwright uses strong purgatives of calomel, aloes and scammony (as twelve grains of calomel, and eight, each, of aloes and scammony); while Dr. Manson says "*hypercatharsis* is often induced by the *mildest aperients*," etc. Unless Dr. Cartwright has greatly improved, since the date of his Essay, we think that few will deny that Dr. Manson is by far the better practitioner of the two. But, be that as it may, we should like to know of these writers, whether pneumonia, when met with in negroes at the South, is to be cured by other remedies than those given to whites, when sick of the same disease; for neither in Dr. Cartwright's Essay on "*Pneumonia Biliosa*," nor in Doctor Manson's Essay on "*Malarial Pneumonia*," is there any modification of treatment suggested for the two races in question. We take it, that all sensible men will adapt their remedies to

the patient's *condition*, whether that patient be white or black, or of any intermediate shade of color,—well knowing that the name of a disease is not a safe guide to its treatment. We perfectly agree with Dr. Cartwright, that our medical journals, containing the experience of our enlightened physicians, "are of more value than what the auscultators in London and Paris *hear*," or what auscultators in the South, or anywhere else, "*hear;*" but if he means by this to allege that, in the thoracic diseases of the South, the auscultatory phenomena, in the same diseases, are not identical with the auscultatory phenomena in London, Paris, and elsewhere, we deny the correctness of the statement, *in toto*, and call for the evidence, if there be any, upon which it is made. We strongly suspect that Doctor Cartwright ventured this allegation, less upon his own experience than upon Doctor Manson's account of the *physical signs* of his "malarial pneumonia;" and it was because of our apprehension of the evil influence which that account might exert, that we devoted so much space, in our Review, to its refutation. Whether we succeeded or not, it is for others to determine. In this connection, however, one cannot help remarking, as not a little singular, that, while Doctor Cartwright was denouncing European medical writers, as authorities not to be respected in diseases of the South, he should have forgotten, or not have observed, that European writers are the only ones to which Dr. Manson refers in the essay under consideration, and that Dr. Manson ignored even the authority of Dr. Cartwright himself. The "*interesting letter*" had not then been written. But the fact is, that, notwithstanding the denunciations of the authorities in question by a certain class of writers here in the South, they do not hesitate to prop themselves up by confirmatory references to eminent authors in Europe, the North, or elsewhere, when, by so doing, they hope to give currency and consequence to their own performances.

3. In regard to Doctor Cartwright's affirmation, that the great majority of Southern city practitioners are either servile submissionists to European medical authority, or arrant quacks, who, not being able to cure the diseases of negroes, to avoid starvation

in the country, have fled to the cities, where, by doing a little surgery, editing medical journals, republishing European books with stupid notes and commentaries, and getting professorships in medical schools, they are able to exist, it can be hardly necessary to observe that such grave charges are so transparently absurd, as not to be worthy of serious refutation. How many physicians in Southern cities edit medical journals? and of these, how many deserve to be placed in Dr. Cartwright's catalogue? How is it in his own city? Are Dr. Bennet Dowler — the editor of the *New Orleans Med. and Surg. Journal*,— and Dr. E. D. Fenner—one of the editors of the *N. O. Med. News and Surg. Gazette*,— of the class described? And how many Southern city physicians have republished European works, with notes and commentaries, either stupid or wise? How many occupy chairs in our Medical Colleges? Lastly, how many physicians can live in a city "by doing *a little surgery?*"* To ask these questions is to answer them. Such statements amount to less than nothing. They serve only to show the weakness of the cause. Surely, Dr. Cartwright's letter, however "interesting" *to Dr. Manson*, could not have been intended for publication.

III. Finally, we come to "the *conspicuous part* which Doctor Cartwright has enacted in the introduction of quinine as a sedative in fever, &c.,—*the great medical fact of the nineteenth century*,"—upon which "rest," Dr. Manson conceives, Doctor Cartwright's "*highest claims to distinction.*" Dr. Cartwright says to Dr. Manson—

"*You were pleased to ask me 'who first used large doses of quinine in the febrile exacerbation in the South.' Put* WORLD *for* SOUTH, *and I answer, that the records of Medicine do not furnish any example of quinine having been used in large doses anything like as far back as April*, 1826, *when I wrote the article on pneumonia biliosa, strongly recommending the sulphate*

* We apprehend that if country physicians could be induced to believe that they could get along in cities, the country would soon be very nearly depopulated of doctors. The exodus of doctors from the country, however, is not likely to be great, if they bear in mind Doctor Cartwright's statement respecting Dr. Bouldin, who, not succeeding in the city of Natchez, settled in the country, where his great success introduced him to a large and lucrative practice.

of quinine in large doses in that disease, with the single exception of a paper published by Dr. Perrine near about the same time, in Chapman's Journal. I may here observe, that Dr. Perrine was a protégé of mine, who came from Illinois to Natchez. Dr. James A. McPheeters and myself indoctrinated him into the mysteries of treating fevers and pneumonia with quinine."

Whether Dr. Cartwright was the first to use quinine in large doses, during the existence of high fever, we know not. It is not a matter of much practical interest at this day, when the use of quinine in large doses is well known to everybody. But it is certain that, if Dr. Cartwright is justly entitled to the honor of having been the first to elucidate the great therapeutical principle in question, he has not succeeded in making it generally known in the South, yea, not even to a distinguished *Quininist* (Dr. E. D. Fenner) in his own city.

1. Let us see how the matter stands in this country. In his report on the diseases of Jackson, Mississippi, Dr. S. C. Farrar states—

"So far as I am informed, the credit of exhibiting quinine in twenty and thirty grain doses, in the fevers of the South, is due to Drs. Perrine and Thomas Fearn, distinguished practitioners of Medicine, formerly of the State of Alabama; * * * and the late Dr. M'Pheeters, of Natchez, gave quinine in large doses at a very early period after its discovery."

To this statement, Doctor E. D. Fenner appends the following note:

"We may take this occasion to inform the respected author of this report, that the Dr. Perrine he has mentioned was not a practitioner of Alabama, but of the vicinity of Natchez. So early as 1823 he gave ten grains of quinine at a dose, *without regard to the exacerbation of fever*, and with happy results, as is testified by Dr. Cartwright in his paper on the yellow fever of Natchez, published in the Medical Recorder of that period. Doctor Perrine published his experience in the Philadelphia Medical Journal, about the year 1826. If we are not mistaken, Dr. Fearn, of Huntsville, Alabama, gave his first large doses of quinine in 1829 or 1830. From a recent conversation with Dr. James Metcalf, who resides in the vicinity of Natchez, and is one of the oldest living practitioners of that region, we have learned that, about the year 1820, he had adopted the practice of *giving large doses of quinine in the exacerbation of fever, and with the happiest effects.* Dr. M. says he taught the practice to Dr. Perrine, who lived in his neighborhood. Dr. Metcalf is now a wealthy planter of high standing, and is probably the first physician, either in Europe or America, who gave large doses of quinine in the hot stage of bilious fever, with the view to *cut short* the disease."*

* Fenner's Southern Medical Reports, vol. i. 352.

Now, here is an issue of fact between Dr. Cartwright, Doctor Fenner and Doctor Metcalf. Doctor Fenner thinks that Doctor Metcalf was the *first physician, either in America or Europe*, who gave large doses of quinine in the hot stage of fever, and Dr. Metcalf says *he* taught the practice *to Dr. Perrine;* and yet Doctor Cartwright alleges that Doctor McPheeters and himself indoctrinated Dr. Perrine into the mysteries of treating fever and pneumonia with quinine! So far as the use of quinine in the *inflammatory affections* of malarious districts is concerned, Doctor Baldwin, of Montgomery, Ala., alleges that, to the late Doctor Boling, of that town, is due the credit of having been the first, in this country, to give *a definite direction* to this subject (*a*). It is thus seen, that there are several claimants to the honor in question, and that, by those who ought to be correctly informed in the premises, Dr. Cartwright has not been reckoned among the number. There is some doubt about the matter, at all events, in respect of this country.

2. We will now see how Dr. Cartwright's claims will square with the facts, in regard of certain *European authors.* It will be borne in mind, that Dr. Cartwright affirms, that the *records of Medicine* do not furnish *any example* of quinine having been used in large doses *anything like as far back as April*, 1826, when he wrote his article on Pneumonia Biliosa, with the *single exception* of Dr. Perrine's paper, published near about the same time. It is very true that, in the essay to which he refers, Dr. Cartwright did, in one case, give eight grains of quinine at a dose; but if we have correctly apprehended the purport of his paper, he advises that quinine be used chiefly in the *cold stage* of pneumonia biliosa, in doses of *four* grains every one or two hours, for the purpose of *bringing about* a general reaction, and of preparing the system for the employment of other agents, particularly *purgatives* (*b*). But be that as it may, we are informed by Dr. La Roche that the use of quinine, in large doses, as a powerful and pure counter-stimulant or *sedative*, originated in *Italy* some thirty years ago. In England, Dr. Elliotson gave

(*a*) American Journal Medical Science, vol. xiii. 305.
(*b*) The Medical Recorder, vol. x. 57, 75–77.

it in typhus fever, in *five-grain* doses, on the 19th of June, 1823; and in remittent fever, in the same doses, on the first of September of the same year, notwithstanding the existence of *great feverishness, thirst, and a very quick pulse* (*a*). In the latter part of August, 1822, M. Bailly gave at Rome, to an Irish priest, twenty grains of calomel and *ten* of quinine, in two doses, at an interval of two hours, and subsequently, *every two hours* during the day, *five* grains of quinine; for a woman, ætat. 21, he directed *sixty grains of quinine in two days;* to a soldier, ætat. 36, he gave quinine in doses of *twelve grains;* and he says that the doorkeeper of St. Peter's church took *a hundred grains of the sulphate of quinine in four or five days* (*b*). And yet Dr. Cartwright affirms "that the *records of Medicine* do not furnish *any example* of quinine having been used in large doses *anything like as far back as April*, 1826, when he wrote the article on Pneumonia Biliosa, strongly recommending the sulphate of quinine in that disease, with the *single exception* of a paper published by *Doctor Perrine* near about the same time"! Few men are in a condition to make such sweeping statements respecting what "the records of Medicine *do not* furnish;" and Doctor Cartwright has himself given us conclusive proof, that he cannot be reckoned among that small number. The facts adduced show, that, in regard to the use of quinine in large doses, Doctor Cartwright was anticipated *six years* in this country and *four years* abroad; and in following the evidence presented in the case, "the Doctor's *highest claims to distinction*" become "small by degrees and beautifully less." Never before Doctor Cartwright's very modest presentation of his own pretensions, in the "*interesting letter*" to Doctor Manson, and Dr. Manson's flourishing endorsement of the same, have we ever seen any mention made of "the conspicuous part which Dr. Cartwright has enacted in the introduction of quinine as a *sedative* in fever;" and we are yet to be convinced of the fact, if, indeed, fact it be. But certainly, had Dr. Cartwright been familiar with eminent European authors, whose writings, as unworthy the respect of

(*a*) Elliotson's Practice, 277, 78.

(*b*) Traité Anatomico-Pathologique des Fièvres Intermittentes, etc., 326, *et seq.*

Southern physicians, he so much derides, he would have saved himself the very unpleasant *faux pas* of setting up claims in his own behalf, in which European authors had preceded him by some years; and had Dr. Manson been more familiar with the works of the "British and Continental writers," to which he makes reference in his Essay on "Malarial Pneumonia," he would not have exhibited the *faux zèle* of endorsing claims that "rest" upon so sandy a foundation. Indeed, we have the authority of a distinguished Southern physician and teacher, Prof. S. H. Dickson, that "it is in *France* especially that the use of this most valuable medicine (quinine) has been extended;" and Prof. Carson (than whom there is no more competent judge) affirms that "it is with no fear of contradiction that we state the work of M. Briquet (*a*) to be the most complete and satisfactory that has yet appeared with respect to the action of bark, or of its preparations upon the economy." And so far as the use of quinine in *sedative* doses is concerned, Doctor La Roche assures us that "the practice is evidently but a revival of the one suggested and resorted to as early as the close of the *seventeenth* century, by Morton,—adopted not long after by Torti, Burserius, Werlhof, Truka, and other physicians of the times, and highly eulogized, so far at least as regards its applicability to yellow fever, by Arejula, Sarravia, Lafuente, Bobadilla, and the majority of Spanish physicians, as well as by Valentin, Cassan, Lefoulon, Guyon, Savarési, Stevens, Kuhn, etc.—of administering the *Peruvian bark in the largest possible doses*, in all stages of the disease, after little preparation, or without any preparation at all. * * * * * Forget for a moment that the writers cited are speaking of cinchona, and not of quinia, and you may fancy that the latter remedy is the subject of their remarks. Upon the administration of a large dose, it was said, *the pulse is reduced; the skin cools and moistens; thirst, if it existed, disappears; the tongue becomes clean and moist; in a word, fever disappears as if by enchantment;* and if the patient experience a little uneasiness about the head, a little dizziness, perhaps a

(*a*) Traité Thérapeutique du Quinquina et de ses Préparations. Par P. Briquet, Médecin à l'Hôpital de la Charité, &c. Deuxième édition. Paris, 1855.

slight buzzing in the ears, the whole soon subsides, and convalescence follows" (*a*).

In conclusion we will say, that we have not the smallest disposition to detract one jot or tittle from Dr. Cartwright's just claims to distinction. Nevertheless, we avow our adherence to the teaching of the golden rule,—*tribuito suum cuique.*

OPERATION FOR SOFT LENTICULAR CATARACT BY DIVISION.

Reported by James F. Long, *M.D., of Washington, N.C.*

Charles Moor, a negro man, aged about 50, came under my observation last June, with loss of vision in both eyes, in consequence of complete lenticular opacity, resulting, as he expressed it, from an itching humor in the head — erysipelas, I suppose. The opacity was of a glistening, grayish-white color, and presented all the peculiarities of soft lenticular cataract. The eyes were otherwise quite healthy, the integrity of the retina preserved — as was evinced by perception of light and darkness — and the patient's general condition good.

The indications for an operation being thus favorable, the patient desirous of relief, and willing to submit to anything that would promise good, I decided to operate, chose the left eye, for the sake of convenience, and division as the most eligible mode.

Certain matters requiring the personal attention of the patient, made it necessary to postpone the operation until the 3d of September: accordingly, at that time, the patient having been for a few days previously confined to his room, and prepared for it properly, I proceeded in the following manner to operate:

Taking care to dilate the pupil with extract of belladonna, the patient was seated on a chair, facing a good light. My student, Mr. Redding, standing behind him, supported his head, and retracted the upper lid: using my left hand to secure the lower lid, I, with my right, entered a curved lance-head cataract needle into the sclerotic coat, about three lines from the temporal margin of the cornea. On the disappearance of the head, the

(*a*) Op. cit., 475.

direction of the instrument was changed, and it was passed on behind the iris, directly into the posterior chamber of the eye, so that its head was seen through the pupil; then, by movements of the fingers, it was made to lacerate and freely divide both the lens and capsule: the instrument was then, in a reversed manner, carefully withdrawn, and the opaque substance left to the solvent influence of the humors of the eye.

About five days after the operation, considerable posterior inflammation having supervened, cups were applied to the temporal region, and the system brought slightly under the influence of mercury, which treatment promptly controlled it. By the daily use of laxatives, and the utmost care as to diet and exposure (the patient being kept in a dark room, the lids closed, and secured by strips of black plaster, and over all a dark shade adjusted), the case has progressed very favorably; the opaque substance has been gradually absorbed, leaving the pupil quite clear, looking natural, vision restored, and the patient desirous of a similar operation on the other eye.

LITHOTOMY IN THE FEMALE.

BY DR. C. WINSLOW.

In attempting to refresh my surgical knowledge a little, with a view to reporting a case of lithotomy in the female, I find the operation condemned at the very outset. Dr. Henry H. Smith, in his Operative Surgery, which work I happen first to refer to, purposely omits any description of it, and remarks that, "as all calculi can now be readily removed by crushing, I do not regard the operation of lithotomy in the female as justifiable."

While hesitating to differ from authority so competent, I yet can readily conceive of a class of cases, in which lithotomy affords the *only* means of removing a stone from the female bladder, and of another class in which it would be the *preferable* mode, although lithotripsy might be *possible*.

Incrustations of stone closely adherent to the lining membrane of the bladder, and requiring to be forcibly peeled off with the

finger, or with an instrument, as in the case operated on by Dr. Sapplington,—encisted stones also adherent, and requiring to be forcibly torn from their beds, as in the case of the young man Webb, reported in a former number of this Journal,—very large and hard stones occurring in young subjects, with small urethras,—belong to the first class of cases; while in the latter, I should place nearly all cases of stone in very young subjects, and those cases, whether occurring in young or old, in which the stone nearly or quite fills the entire cavity of the bladder, accompanied by great irritability of that viscus, in which lithotripsy would have to be very often repeated, before reduction could be effected.

In the case which I am about to report, I am not certain whether or not lithotripsy was practicable, but I am very well assured that lithotomy was preferable.

In September, 1854, while riding by the residence of Colonel Felton, of this county, he requested me to call to see a negro child of his, who was then enduring extreme agony, and whose screams were intolerable to his family. I found a little girl, five years of age, badly developed, and much emaciated, who, ever since her birth, had exhibited symptoms of pain, which, for the first two years, were thought to be colic, but which, as she grew older, were referred to her bladder. Difficulty of micturition, and intense suffering after the attempt,—a constant stillicidium of urine,—the resultant offensive odor, and a tumid abdomen,—were the symptoms most prominently pointing to stone in the bladder. Her pain was of that peculiar bearing down character, as though she were about to evacuate the whole pelvic contents, which characterizes the existence, and so intensifies the suffering of this malady in the female.

I introduced into her urethra a silver probe, taken from my pocket-case, for want of a better instrument at hand. This came at once in contact with the end of what appeared to be a small spiculum of stone, which, having entered the canal, had advanced nearly to its outlet.

Thinking I had to do with a small fragment that could be easily removed, I seized its apex with a bull-dog forceps, gently,

lest I should push it back into the bladder, and attempted to withdraw it, by long-continued traction. I soon discovered that the attempt was futile; nor was I more successful when I endeavored to push it back into the bladder; for although it receded to some extent under pressure, as soon as the force was withdrawn, the stone resumed its original situation. This effort caused the child much suffering. I administered a full anodyne, and had her conveyed, while under its influence, to my residence.

A preparatory treatment was instituted, with the view to allay the general irritability of her system, and especially the highly-excited condition of her bladder and its appendages. Her bowels were thoroughly evacuated by repeated doses of a mixture of calomel, castor oil, spirits of turpentine and oil of wormseed. She was kept in bed, with large flaxseed poultices to her abdomen and vulva,—allowed free diluent drinks and generous diet,—while her suffering was allayed by frequent doses of a mixture of morphia, chloroform, bicarb. soda, and syrup of acacia.

Late in the afternoon of the 24th, I proceeded to make a more thorough exploration of the child's bladder. On this occasion, I had the benefit of the intelligent advice and assistance of my friend and former pupil, Doctor David Cox, who put our patient thoroughly under the influence of chloric ether. Without the anæsthesia, the sensibility of the parts, although diminished, would not admit of a satisfactory examination. I introduced the forefinger of my left hand into the rectum,—the vagina being too small to admit it,—then taking firm hold of the stone with my pocket forceps, in my right hand, and making traction and rotation, the fact was quickly revealed to me, that nearly the whole cavity of the bladder (which was considerably reduced in size), was occupied by a stone, of which the part within the grasp of the forceps was only an offshoot, moulded in the urethra.

It was very apparent to us that the stone could not be removed entire, and we resolved, after incising the urethra, to introduce the large crushing forceps, and reduce it to fragments. In order to avoid, if possible, incontinence of urine, which would render life scarcely desirable either to the patient or her master, we determined to leave the sphincter vesicæ uncut, as recommended

by Liston, to divide the urethra by three very shallow incisions upwards and laterally, and by persevering dilatation, to introduce the forceps into the bladder. By this process we avoid, as far as possible, the dangers of incontinence of hemorrhage, and of wounding the vagina.

The crushing forceps used on this occasion measured two inches around the blades when closed,—have wedge-shaped teeth for splitting the stone, and powerful leverage. As a catheter could not be passed into the bladder, we did not attempt to inject it.

Early next morning, having again put our patient under the influence of chloric ether, a narrow probe-pointed bistoury was passed, flatways, between the spiculum of stone and the upper part of the urethra, for the distance of three-fourths of an inch; then, turning the back of the blade towards the stone, the upward cut was made in withdrawing the instrument, deepening the incision from within outwards. The lateral incisions were made horizontally, in the same way.

I now introduced a small polypus forceps, partially expanded, so as to receive within its grasp the part of the stone lying in the urethra. This part was broken off and extracted. The canal being now free, I oiled first the little finger, and then the forefinger of the left hand, and passed them successively into the bladder, until, by gradual dilatation, I was enabled to introduce the crushing forceps. With these I broke down such small ledges of stone as I could grasp. These being removed with smaller forceps, the crushing process was repeated until the whole stone was reduced to fragments sufficiently small to admit of easy extraction. The smaller fragments and sand were carefully removed with the scoop, after which the bladder was repeatedly washed out with warm water, forcibly injected, so as to flood away, if possible, all the sediment. This was done, especially, in order that the foreign particles might not lodge in the incisions and retard their union. An elastic catheter was now fastened in the urethra, for the double purpose of preventing distension of the bladder, and of insuring accurate apposition of the edges of the incisions. Three points of suture—one for each incision—were now introduced, so as to cause the urethra

closely to embrace the catheter. Our patient's thighs were closely bandaged together; she was put under the influence of morphia and tartar, and directed to be kept upon her back.

The operation was necessarily tedious; for although the power of the forceps enabled me to crush, by a single effort, whatever came within its grasp, yet the large size of the stone, and the limited extent to which I could expand the blades, rendered the early part of the process a series of nibbling, rather than crushing.

Scarcely any fever followed the operation. On the third day the sutures were carefully cut and the catheter withdrawn; a slight flow of blood followed, the result, I believe, of some laceration, caused by a fragment of stone lodged in the eye of the catheter. A smaller instrument was allowed to remain in the bladder a few days longer, at the expiration of which time my patient was well. On the sixth day after the operation she was sent home, cured. Since that time I have frequently seen the child, and interrogated her parents and herself, as to her control over her bladder. She has no incontinence of urine, and is now stout and healthy.

The stone has not been analyzed. Many of the smaller fragments and the sand were not saved. Those collected weigh 490 grains.

The points of interest in this case I conceive to be—the relatively large size of the stone; the mode of incising the urethra; the leaving the sphincter uncut; the use of sutures in closing the incisions, and the rapidity and completeness of recovery.

In conclusion, I wish distinctly to disclaim any pretension to originality in any single step in this operation. Each one has been practised or advised by the great pathmakers in Surgery. I can only felicitate myself upon having used, with the most fortunate result, the pathways which they have marked out.

PERTUSSIS—OXIDE OF ZINC.—In the latter stages of whooping cough I find the oxide of zinc, in connection with the sulphate of quinia, quite valuable. Small doses seem to act better than larger ones. Tannic acid is also frequently indicated.—*Dr. Miller.*

REPORT ON THE DISEASES OF DAVIE COUNTY.

Presented to the State Society by W. R. SHARPE, *M.D.*

Mr. President:

IN attempting to discharge a duty which you did me the honor to make obligatory, at the last annual meeting of the Society, I am not insensible of my inability to produce anything, either valuable or interesting. Nevertheless, with your indulgence, and the forbearance of my fellow members, I will make a few remarks, first, upon what I am pleased to call—

AN EPIDEMIC ERYSIPELATOUS INFLAMMATION OF THE BRAIN, OR ITS MEMBRANES.

This epidemic appeared among us first in March, 1856, succeeding the first of the two cold winters, which, by their severity, has distinguished them from any others for the last fifteen years. This epidemic, to me, is striking—First, from its novelty; secondly, from its fatal tendency; and thirdly, from the rapidity with which it runs its course. The same disease prevailed in the spring of 1857, but was much milder and more manageable than it was on its first appearance. According to my observation, the age most liable to be attacked was from fifteen to twenty-two years old; there were many younger, but not many older, persons attacked. Children from two years old and upward were not exempt.

In some instances, the attack was sudden, the patient at once complaining of violent pain in the head, and in a short time would, in most cases, be insensible, and in a few hours fall into convulsions (after the latter of which a recovery was very rare), death following, in many cases, in from six to twelve hours. In other cases, especially among those from fifteen to twenty, or twenty-two years old, the patient would complain of slight pain in the head, with some indisposition, for three or four days; and then, suddenly, would complain of severe pain, and probably fall in attempting to walk from the fire to the bed, and in a short time be insensible, either talking incoherently or lying in stupor. In this condition he might remain for twelve or eighteen hours; but in many cases convulsions ensued earlier than this,

and even death. If called early, on approaching the patient, you would probably find him with flushed cheeks, full and bounding pulse, with one eye looking a little watery, and its conjunctiva slightly injected and inflamed. In most cases the patient would describe the pain as being confined to one side of the head, in one temple, or over one eye. If the treatment is about to prove successful, or the case terminate favorably, about the fifth or ninth day the patient may complain of violent pain in one ear; or the eye, and the side of the face corresponding with that side of the head which has been complained of, will become swollen. Either of these (and especially the latter), I hail as a favorable omen, as it is evidence that the brain and its membranes are being relieved, and the disease is becoming external. The swelling of the face, or pain of the ear, soon subsides, and the patient is convalescent. The swelling of the face (which is erysipelatous in its nature) is one of the best proofs that the disease of the brain is erysipelatous in its character. After the convulsions come on there is no flush of the cheek, and the temperature of the surface but little, if any, elevated; but often cool and pale, and the pulse but little affected, except in frequency. I have known a young, and apparently healthy and cheerful lady, take a ride in the morning, return, and before twelve o'clock complain of some pain in the head, and in a few hours become unconscious, and before midnight was a corpse. I have also been called to a young man who had complained for three or four days of some pain in the head, seemed indisposed to move about or take any exercise, and pay but little attention to what was passing about him, yet give no uneasiness to his friends or family, until, on the third or fourth day, he suddenly complained of violent pain in the head, fell helpless and insensible on the floor, and who I found, on arriving, having one convulsion after another, and before low twelve was silent in death.

This disease struck terror to the neighborhood where it first appeared. I was myself shocked and confused with the first few cases, which appeared as sudden and overwhelming as an avalanche. I have seen this disease commence with the father,

and in four days attack six children, one after another, leaving only the mother and sucking child untouched—each appearing to be in perfect health an hour before the attack. This aroused me, and caused me to adopt the very active treatment which I shall hereafter notice. I have not been able to learn that this epidemic appeared in any part of the country prior to its breaking out in this county. The northern part of this county, which did not suffer from this epidemic in the spring of its first appearance here, was attacked in the early part of the winter following. In the county north, and immediately above the latter neighborhood, in the spring following, it proved quite fatal. This same epidemic prevailed in the bounds of my practice in the spring of 1857; but mostly in those neighborhoods which were in a measure exempt during the spring of its first appearance, and was of much milder form, showing itself externally, by swelling of one side of the face, in many cases from the onset of the disease. It was now very managable; and I do not now remember of having a single case among those families that were attacked in the spring of 1856. This disease differs from ordinary or idiopathic inflammation of the brain or its membranes — first, in that it prevailed evidently as an epidemic; secondly, that only one side of the head in most of the cases was complained of; thirdly, that there was swelling of one side of the face in very many cases, as the disease was giving way, and in some from the onset, and especially so as the epidemic was subsiding; and fourthly, that the very hot and dry skin which is very rarely wanting in the first stage of ordinary inflammation of the brain, was rarely, if ever, found in this disease.

I have not been able to learn much of the treatment of this epidemic by the various practitioners who have had it to treat; but, so far as I have learned, the treatment has been various, and the success equally so. I must, therefore, be permitted to give my own treatment of this disease, as it appeared during the first spring of its prevalence (viz., 1856).

When called to children, if I found them rational, I usually commenced the treatment with an emetic of ipecac., and followed this with a large dose of rhubarb and calomel; directing the

dose to be repeated or followed by castor oil in a few hours, if the first did not act promptly and actively. After this, I kept up pretty active purgation with calomel and rhubarb, and gave sudorifics in the interval, until the disease gave way. If I found the little patient, as I frequently did, insensible, I at once gave it a large dose of calomel, and followed this in two hours with as much croton oil as I thought it would bear. This, if the child had not fallen into convulsions, would rarely fail to restore it to consciousness. Then a milder course of purgation was continued until the little patient was convalescent. Adults, and those who were over ten or twelve years old, if the cheeks were flushed, the pulse full and strong, and a good deal of heat of surfaces, I frequently bled; but I found it important to be very careful in the selection of cases for the lancet. To bleed while the surface was cool, although the patient was insensible, was apt to prove injurious, if not fatal. Where the attack seemed violent, and especially if I found the patient talking incoherently, or lying in a stupor, whether I bled or not, I gave the patient at once a full dose of calomel, and followed it in two or four hours with as much croton oil as I thought he or she would bear. This I repeated sooner or later, according to the symptoms, but always by the next day. I think I never found it necessary to give the croton oil after the second day; then a milder course of purgation with calomel and some vegetable or saline purgative was continued, with tartar emetic or sudorifics. I could not discover that the cases which I bled did any better than those I did not; but when I did not bleed, the more active purgatives were used. I soon learned, therefore, to bleed but little in this disease. Active purgatives, as you will see, were my chief reliance. After I had fully adopted this course, and diligently and perseveringly carried it out, I had no reason to change the treatment, as it was entirely successful. So far as I have been able to learn, those who adopted a milder and less active course were less successful. My motto was a through ticket and a free passage on the express of the great alimentary canal. Where convulsions had supervened before I saw the patient, I at first bled, cupped and scarified; gave croton oil,

and blistered the occiput, but never with success, as I now remember. I finally made it a rule to do but little when convulsions had fully set in. No case in my practice lasted more than twelve hours, after the convulsions came on.

The second epidemic, or that which prevailed in 1857, was much milder, and consequently required a much less active treatment; but even here I found purgative doses of calomel a very important part of the treatment. In no disease which I have ever treated, have I found calomel and active purgatives so imperatively demanded and so well borne. As tincture of chloride of iron is acknowledged to be a very important medicine in the treatment of ordinary erysipelas, it may be asked why I did not adopt this as one of the chief medicines in the treatment of this disease, which I considered to be erysipelatous in its character. My answer to this would be, that owing to the delicate structure of the parts attacked, their vital importance to the economy of life, the violence of the disease, and the rapidity with which it moved towards a fatal result, I feared to trust a medicine so slow in its effects. I believe I might have used it with advantage during the epidemic of 1857, when the disease was milder and less rapid in its course. The treatment, however, which I adopted after the first two or three cases in the previous epidemic, was so satisfactory in its results, that I had no reason to change it, but simply to modify, according to the less severity of the case or the strength of the patient.

I have said what I have to say at present on erysipelatous inflammation of the brain; but I am unwilling to discontinue this report until I have made a few remarks upon some anomalies which appeared in that part of my county and the adjoining county, comprised in the valley of the Yadkin, in the pneumonia which proved so fatal during the winter of 1849. And I will mention in the first place that the previous autumn was replete with bilious intermittents and remittents, consequently the pneumonia which followed showed marked evidence of great congestion in many of the cases. Many of the cases were of the ordinary character, and treated in the ordinary way. Among these were many, in which the initiating chill was of unusual

severity and length, the patient complaining during the chill of violent pain in some part of the chest. After reaction had taken place, the patient would be apparently in a stupor, and seemingly unconscious of what passed around him; yet in many cases might be aroused so as to answer a few simple questions; in others, the coma was complete. He lay with his mouth open, his tongue dry as a board, and covered with a thick brown coating; his eyes shut, and breathing with a snore, or a kind of snort, as though he were lying in a fit of apoplexy; the pulse full, but little, if any, increased in frequency; the face livid, not flushed; the hands and feet cold, the respiratory murmur increased over one lung to that of puerile respirations; while the respiratory murmur on the other side was indistinct, with some crepitus. The patient would make no complaint whatever.

There was evidently very considerable congestion of the minute air-passages of the lungs still remaining. Thereby the blood, in its passage through the lungs, failed to come properly in contact with the air, and not receiving its necessary supply of oxygen, was unfit to supply the proper stimulus to the brain, and thereby keep up healthy action of that organ, consequently the stupor that has been observed. During this time there was no cough, nor expectoration. These patients bore the lancet pretty well; but I always gave large doses of carb. of ammonia and gum camphor, in combination with calomel and Dover's powder, immediately afterward. This, in the course of twenty-four hours, usually relieved the congestion; the patient would now breathe much easier, the lividity of the countenance pass into a flush, the partial fever subside, and coughing, with free expectoration, come on. The patient would now bear tartar emetic, and other remedies usually given in pneumonia.

But this is not the character of the disease to which I wish more particularly to call your attention. The cases which I more especially wish to bring to your notice were characterized by a want of reaction after the initiating chill, until the third day. The chilly sensation would pass off in a few hours, but there would be no return of the natural warmth of the body, the skin

would be cool, white, and as smooth almost as polished marble; the patient's mind perfectly clear and unclouded; the pulse small, and sometimes almost imperceptible; breathing difficult, and great pain in some portion of the chest, and frequently great complaint over the region of the spleen. There is no expectoration during the cold stage and but little cough, and great pain in attempting to cough.

In some few cases there was excessive vomiting, and a discharge from the bowels of a dark, or green bilious matter, for several hours. In these cases the difficulty of breathing was very distressing; the patient was frequently declaring that he would suffocate. In those cases where the congestion seemed to be only passive, the difficulty of breathing not great, the pulse not greatly depressed, the patient's mind clear, and the stomach and bowels undisturbed, I dry-cupped over the parts complained of, used warm poultices, gave broken doses of calomel combined with quinine, and large doses of carb. ammonia and gum camphor in the intervals, often adding to these either opium or Dover's powder. By the third day a flush on the cheeks would appear, the warmth of the surface return, the pulse become a little fuller, general symptoms of fever would come on, and the patient would now cough and bring up bloody sputa. At this point it was necessary to change the treatment, and assume the ordinary treatment for pneumonia, only in a less active degree than in the purely inflammatory cases.

None of these cases proved fatal in my practice; but all had early attention. In the cases of more active congestion, where the difficulty of breathing was very great, and especially where there was bilious vomiting and purging, I gave hot brandy toddy and laudanum, giving between these quinine, carb. of ammonia and gum camphor, at the same time applying warm poultices of mustard to the chest, hypogastrium, and the extremities. The reaction came on sooner than the third day, if at all; and I do not now remember of losing one of these cases. Another striking anomaly in this disease was that in which some of the patients complained of nothing but a strictly periodical and severe pain in some part of the head, usually over one eye, or in one temple.

This was greatly increased by bleeding, in one or two instances where I made the experiment.

This case was cured with quinine and opium, giving the quinine during the intermission, and the opium just before the paroxysm, premising with a pretty active dose of calomel.

During that winter I learned of some very sudden deaths from pneumonia, some of them in a very few hours after bleeding.

You will perceive that I have here given three classes of cases, as having occurred in connection with ordinary pneumonia. The symptoms of the first class of cases will strike you as a succinct delineation of the symptoms of what has generally been called Typhoid Pneumonia, yet these, in my opinion, were not typhoid cases. There were no typhoid symptoms after the first few doses, and the cases recovered too soon and promptly for typhoid cases; although there were some cases which I considered at the time as purely typhoid, which I have not described.

The second class of cases, as described, readily suggest to me, and perhaps may to you, the nomenclature of Congestive Pneumonia, than which there is nothing more appropriate, in my opinion. This term corresponds with congestive fever, as mentioned by Dr. Dickson, in his work on Practice; yet he objects to the term, "as denoting a separate and distinct type of fever," but does not object to it "as a qualifying adjective, significant of certain contingencies which modify the course and symptoms —perhaps of all—certainly of most types of fever."

The third class was not properly Pneumonia, but occurred in connection with it, and was produced, no doubt, by the same causes.

These anomalies, as you are well aware, have been more fully described by many writers, and in very graphic language, in a Report of the Committee of the Massachusetts Medical Society, as occurring chiefly or exclusively among females, as noticed by Dr. Dickson in his work on Practice, and also described by himself, and afterwards by Dr. Gibbes, of Columbia, as having occurred in South Carolina in 1815 and 1816.

To go further into the history of these anomalies, as having occurred in pneumonia, in various places, and at various times,

as described by various writers, would be tedious and burdensome to you, as well as to myself. Brevity being a necessary object in a report like this, I have dealt succinctly with the diseases which I have brought to your notice.

VALERINATE OF STRYCHNIA.

BY R. WYSONG, M. D., OF CHARLOTTE, N. C.

THE above compound, so far as my knowledge goes, has never been introduced into the pharmacopœias, either of this country or of Europe: I feel, therefore, some delicacy in bringing it before the profession; but believing, as I do, that it will prove an acquisition to the already long list of medical preparations, and trusting to the liberality of the members of the profession for a fair trial of this new preparation before they condemn it, I will give my limited experience with it.

I have been using the Val. Strych. some ten months, and find, that it is more particularly adapted to those cases where there is general debility, accompanied with nervous excitability, loss of appetite, indigestion, constipation, depression of spirits, and all the symptoms following more or less on the want of tone in the nervous system. Heretofore, in many of such cases, I have used strychnia, and although they generally improved under this treatment, yet I never met with as complete success with it as I have in similar cases with the val. strych.: hence I am led to believe that, in all such cases as above mentioned, we have in this preparation a very reliable and useful remedy.

The preparation I have been using is prepared by dissolving sulp. strych. in valerianic acid. So simple a preparation can be put up by any physician or druggist. I have been using it in the following proportions:

Sulp. strych. ..... gr. viij;
Val. acid. ..... ℥i;

but this quantity may be altered to suit particular cases.

The following are some of the cases in which, among others, I have used it with entire satisfaction:—

May, 1859.—Mr. E., aged 45—has suffered with asthma for fourteen years; has tried various remedies without the least benefit. The paroxysms came on about once a week, he never passing two consecutive weeks without having one; is very nervous, and suffers from general debility.

Jan. 19th, '59.—Prescribed

℞—Of Sulphate of strychnia, gr. i;
" Pure water ..... ʒi;
" Acetic acid, suf. quan.

Directed ten drops, three times per day, to be increased one drop per day until the dose reached thirty drops. Kept him on this prescription one month without any apparent benefit.

Feb. 20th.—Prescribed

℞—Of Val. strych. ..... ʒi;
" Water ..... ℥i.

Directed ten drops, three times per day, to be increased as above. Has never had but one paroxysm since he commenced with this prescription, and is now, to all appearances, in good health. The medicine was kept up about one month.

Sept. 20th.—Has continued free from asthma; occasionally, during damp or rainy weather, experiences slight hoarseness, with disposition to clear the throat: this is generally relieved by a few doses of the sol. val. strych.

May, 1859.—Mr. R., aged 60—has had asthma since July, 1858, the paroxysms increasing in frequency each week. He now has one nearly every day, and sometimes two per day. In conjunction with asthma, he has bronchitis, also indigestion, torpor of the liver, as indicated by decided yellow hue of the skin, constipation, and ashy stools, he never having an evacuation without taking some purgative: at times there is decided stranguary; this seems to be superinduced by constipation, as it is always relieved by free action of the bowels. Pulse over 100, small, and at times can scarce be counted; great general debility—spends more than half the day in bed; very nervous, so much so, that by reading the least exciting newspaper article, or even conversing with a friend, will at times bring on a paroxysm. Cannot sleep in the recumbent posture, but is compelled

to be propped up nearly straight in bed. The paroxysms come on generally about 3 P.M., but may be brought on any hour, by the least excitement. Has been in the habit of taking nauseants when he feels an attack coming on, which, after several hours, relieves him for the time.

May 13*th*—Prescribed

℞—Of Val. strych. ..... ℥ i;
" Water ..... ℥ i.

Directed to commence with five drops, three times per day—to be increased three drops per day until the dose reached thirty drops. For several weeks had no sensible effect, owing to the dose being too small, and very irregularly taken; in fact, he seemed to be growing worse.

May 31*st*—Prescribed

℞—Of Acetate of squills ... ℥ i;
" Tar. ant. ..... gr. ij.

Directed to begin one hour before the usual time for attack, teaspoonful every fifteen minutes until nausea was produced; at the same time prescribed

℞—Of Val. strych. ..... ℥ ij;
" Water ..... ℥ i.

Directed fifteen drops, three times per day, to be increased one drop per day, as above. After the fourth day, as there had been no paroxysm, directed the nauseous mixture to be left off.

June 11*th*.—Has had no return of paroxysm; no sign of bronchitis; bowels regular; skin greatly cleared up; pulse 70, rather full and strong; general health improved in every respect, so much so, that he drives out several miles every clear day, and sleeps without inconvenience in a recumbent posture. Continued the valerian, thirty drops three times per day.

July 1*st*.—No return of asthma; health very much improved; seems quite free from nervous excitability. Continued the val. strych., as before.

Sept. 20*th*.—Continued free from asthma up to the latter part of August; had not been taking the medicine regularly for some weeks. About this time, during a very damp day, drove several miles to see a neighbor: upon reaching there, sat down in a draft until completely chilled, which brought on his asthma.

Since that time has had one paroxysm. Saw him September 8th: prescribed val. strych., as before. He is now enjoying his usual health.

I have given the above case at some length, in order to show more fully what I conceived to be the effect of the medicine. I doubt very much whether the case will ever be permanently cured, but by the use of the above preparation the disease has evidently been kept at bay, and the patient's general health very much improved; and by long-continued use of the remedy, he may possibly break up all tendency to asthma.

I have used val. strych. with very great success in the treatment of females. One case of inflammation of the ovarian gland followed by suppuration, in leucorrhœa: of course, in such cases, I also applied local means.

A New Treatment for Asthma.—The Medical Reporter says, Dr. Courty, a Professor of the Medical School of Montpelier, France, has been treating asthma by subcutaneous injections of a solution of the sulphate of atropine over the course of the pneumogastric nerve. He makes a puncture on the inner edge of the sterno-cleido mastoid muscle, directly over the sheath of the great vessels of the neck, by means of a trocar and canula, to the distance of fifteen millimetres, and then injects a few drops of the solution, so that it may come in contact with the sheath of the great vessels.

Burns.—M. Franchini states that he has found Cherry Laurel Water of great service, even in very severe burns, while, even when these have been of large extent, it has produced no ill effect. He combines eight parts with one hundred parts of gum syrup (a mixture of tragacanth mucilage and simple syrup), renewing the application every twelve hours, moistening the compress a few minutes before attempting to detach it.

Bibliographical Notices.

Elements of Inorganic Chemistry, including the Applications of the Sciences in the Arts. *By* Thomas Graham, *F.R.S. L. & E.*

The second edition of this invaluable Treatise, edited by Doctors Watts and Bridges, and published by Messrs. Blanchard & Lea, is before us. The deserved reputation of this excellent work has prompted its American editors to a republication of it, and, in so doing, they have done essential service to the cause of Science, for, both to the beginner and the more advanced student, it affords a rich store of valuable and interesting information. There is one particular merit possessed by this work that will commend it to the especial consideration of the public, and that is, its extreme simplicity and conciseness, —a rare merit, by the way, in this age of pretension and pedantry.

We take pleasure in recommending it to all interested in chemical studies, both in and out of the profession, assured that they will be fully repaid for the time expended in its perusal.

On the Difficulties and Advantages of Cathertism of the Air-Passages in Diseases of the Chest. *By* Horace Green, M. D.

Dr. Green evidently believes in the treatment he proposes; such is his faith, in fact, that he seems resolved to force it into everybody, *nolens volens*. For years he has been inserting his Probang into the public throat with a pertinacity which indicates earnestness at least, on his part, and which would have elicited the gratitude of the profession, without doubt, had not the remedy been *pushed* "*ad nauseam*." The truth is, the medical world has become so accustomed to the hobby-riding of the learned Doctor, that it can form no other idea of him than that which is represented by the picture of a venerable Professor astride of a huge Probang, in regular corn-stalk fashion, flourishing along like a second Gilpin, and resolved to be "in at the death," whatever the game that may be started; and so he will be Daguerreotyped to posterity, as sure as a

gun—of the latest and most improved invention. There is no likelihood of his being forgotten either, for he has cauterized his name in the tissue of medical history, and there it will remain *forever—Green*, whilst a single throat exists to proclaim the triumphs of the great "ex-sponger."

But, notwithstanding his monomania on this subject, there is no denying the fact of his great abilities as a medical practitioner, and of the important service he has rendered humanity by the introduction and perfection of his particular method of treatment in diseases of the air-passages. It is true that he has been vigorously and bitterly assailed, and that, as many a true philosopher before him, he has met with opposition from those who should have been first and foremost in appreciating his services, —but, unlike many of the so-called innovators, who have in reality led the van of Science, he has lived to see his views adopted by the ablest men of the times, and to secure, as a reward for his labors, the liberal patronage of a discerning public.

We have said more in this connection than was originally intended. The Pamphlet in question was simply referred to for the purpose of proclaiming the fact, that, in the unfortunate controversy respecting the death of Whitney, with which the readers of this Journal are familiar, Dr. Green has been sustained *in toto*, by Bennett, Rokitansky and Trousseau, the ablest pathologists of Europe, as is shown by their published letters. With this statement, we take leave of the subject forever: "sufficient for the day is the evil thereof."

We shall postpone our notice of other works for a succeeding number, in order to make room for more interesting matter. Authors, editors and publishers, who desire a *candid* and *independent* notice of their works, can easily secure it, by sending copies to our address, either by mail or express.

SELECTIONS.

Practice.

NEW YORK ACADEMY OF MEDICINE.

Regular Meeting, January 4th, 1860. Dr. John Watson, *President, in the Chair.*

The subject of Diphteria was introduced to the Academy by the President, who called upon the members to give their views relative especially to the differential diagnosis between it and croup, muguet, measles, putrid sore throat, etc. He stated that much confusion existed upon this point, and that, in a recent work published by the New Sydenham Society, in which several papers upon the subject, by different authors, were brought together, there was no clear definition given to distinguish it from the diseases he had mentioned.

Dr. Francis referred to the richness of the literature of croup. He spoke in admiration of the writings of Bard and Bailey, of New York, who were among the earliest in this country, and, indeed, in any country, to give a definition to this disease. He regarded croup as an inflammatory disease, and believed in the great efficacy of emetics, and of vitriolic emetics in particular. He had given a teaspoonful of a saturated solution of sulphate of zinc every half hour, and esteemed this treatment very highly.

Dr. Kissam also regarded croup as of an inflammatory nature, accompanied by a disposition of the blood, which favored the deposition of a false membrane, upon blistered surfaces, the edges of a wound after tracheotomy, as well as upon the mucous surfaces. He favored the use of calomel in the early stages of the disease, which, if persevered in, he had reason to believe had prevented the disease from progressing to the more advanced and fatal stages. In conjunction with this treatment, he had used antimonials in small doses.

Dr. Van Buren drew a line between that form of croup which comes on suddenly at night, and readily yields to a mild emetic, and the more insidious form which is attended by an exudation. The first was spasmodic, the latter true croup. He did not

believe in bloodletting, and had not seen any good results from calomel. The use of vapor of warm water he favored, and had derived great benefit in some severe cases from the local applications of nitrate of silver.

Dr. Percy thought there had been an evident change in the type of the disease during the last few years. Croup, he thought, now presented more of an asthenic character than heretofore. In the treatment of the disease he had never used calomel, but he had found most decided beneficial results from frequently repeated doses of alum. This suggestion, he said, he owed to Dr. Miller, and he had cause to congratulate himself upon the prompt and efficient action of this form of medication. Emesis was almost instantly produced, which was not attended by any degree of prostration, as from other forms of emetics. He had also been in the habit of giving veratrum viride to control the circulation, and had reduced the pulse from 150 to 100, keeping it there by means of the veratrum without producing any emetic action.

Dr. Douglas mentioned the division of croup into true and false—the latter being of a spasmodic character, needing little or no treatment; the former of an inflammatory type, requiring active medication. The disease now prevalent he considered as different from either of these. It was a blood disease, and had its differential characteristics which distinguished it from membranous croup. In the one there was a discharge from the nostrils, which was thin, acrid, excoriating the nares, often sanious in severe cases, which was not present in the other.

In diphtheria, the countenance was usually flushed—not confined to the cheeks, as in croupous pneumonia, but suffused; not of a bright red, but of a red tinged with yellow. In membranous croup, the countenance, on the contrary, was almost universally pale. The cough, too, presented marked differences. The sharp, ringing, dry, metallic cough, so prominent in membranous croup, was wanting, the angle having a moist, liquid character in diphtheria. So, too, the respiration, which is labored and very frequent in membranous croup, is not increased to the same degree in diphtheria, the mechanical obstruction not being so great in the latter as the former.

These had been the peculiar differences he had remarked in the cases which had fallen under his observation. In one case which he had recently attended, the respiration was almost natural in frequency, averaging only twenty-four in a minute—the child being nine years old. The pulse maintained a fair ratio to the respiration—about as five to one—but was easily accelerated on the least exertion. There was an evident prostration of the forces, which would not admit of any depletory measures of any kind; and, viewing this disease as a blood disease, he did not think that calomel was admissible, though he could conceive that in membranous croup, with a firm exudation, it might be used to advantage. In the disease we have now under consideration, he esteemed a supporting treatment called for, and placed most reliance upon quinine and iron, in connection with some stimulants and a proper diet.

Dr. B. F. Barker remarked, that when in Albany last year, in attendance upon the State Medical Society, he had seen several cases of diphtheria, which was then prevalent in that city as an epidemic. He recalled one case which he visited with the attending physician, and had seen the child sitting up in bed playing, with intelligence clear, but presenting the symptoms of this disease in a marked manner. He asked the physician his prognosis of this case. It was, that the patient would die within twenty-four hours. He had not seen a case from that time till late in the Fall of last year, when he was called to see a lady in this city, who had been unwell for a fortnight from general debility. He was called at night, in consequence of her being unable to pass her urine. This state had existed for over twenty-four hours, and the hypogastrium, upon examination, was found distended, and tender to the touch. An attempt to pass a catheter gave her great pain, when she was put under the influence of chloroform, and a large quantity of water drawn off. This gave but temporary relief. Opium was given internally and by suppositories and injections, and warm fomentations applied to the vulva, all without bringing any relief. Upon examination the next day, he found the vulva, the labia and meatus covered with a membranous exudation similar to that he had

seen in diphtheria. He immediately applied a strong solution of nitrate of silver, for the purpose of breaking up this exudation. After the exudation was removed, he applied to the parts a saturated solution of the chlorate of potash. He also gave the chlorate of potash internally, together with tonics and good diet. He mentioned this case as illustrative of the fact that this disease does not affect the mucous surfaces of the air-passages alone, but that all mucous surfaces exposed to the air are subject to diphtheritic exudation. He believed that the tendency of the blood, in this disease, was to *disorganization*, and that the vital powers required to be supported by a tonic plan of treatment.

Dr. Gardner resumed the discussion of Diphtherite, by saying that he had seen some cases of this disease, and while he agreed with Drs. Douglas and Barker in the views they had expressed, thought they had omitted two distinct features peculiar to diphtherite; that it was not only epidemic, but eminently contagious, and that a disagreeable diagnostic smell always accompanied the flow from the nose. He thought a vigorous tonic treatment and local applications to the throat were demanded. The membranous exudation, he thought, differed from that of membranous croup, by being less strong and tenaceous, and not ever, in his experience, running down so as to make casts of the bronchi. He trusted that Dr. Jacobi, who had just entered, and who had a large experience in this disease, would give his views.

Dr. Jacobi.—The main thing observed in diphtheria are the symptoms of entire dissolution of the blood. Hence it was difficult to make a correct diagnosis without the membranes being seen, as the general symptoms of the dissolution of the blood are very similar to those in other diseases, presenting the same character, such as scarlet fever, measles, etc. It would be very difficult, therefore, to make a strict diagnosis without the membranes being observed.

The membrane usually is seen on the tonsils and the uvula, but it is not necessarily confined to the throat; he had seen them on almost every mucous membrane of the body, especially on the mucous membrane of the vagina. The great majority of cases of diphtheria are so slight, that there is hardly any necessity for

medical interference; still, not unfrequently they assume a severe and dangerous character.

As to treatment, his experience, especially during the last two or three months, was not much in favor of using caustic to the membrane itself; and if this local medication had any beneficial effects, he thought that they were limited to the portions of mucous membrane on which no exudation had yet taken place, and where it might possibly prevent it.

Internally, the chlorate of potash and iron, especially the fluid preparations of the latter, and large doses of quinia, especially in such cases as set in with fever, were used. Lately, Dr. Jacobi had substituted chlorate of soda for the chlorate of potash. Chlorate of potash requires only sixteen, chlorate of soda only three or four parts of water to dissolve it. He had repeated the experiments made by some French pathologists, and found that the diphtheritic membrane was more readily macerated in a saturated solution of chlorate of soda than of chlorate of potash.

While it was true that no essential difference could be detected between the membranous exudation of croup and that of diphtheria, yet there was a wide and characteristic indifference between the two diseases. *Croup* had nothing to do with dissolution of the blood, while dissolution of the blood was the *main* pathological character of *diphtheria;* it is related, in this rerpect, to scarlet fever, measles and typhus fever, all of which present this general morbid condition of the system.

Dr. Douglas inquired of Dr. Jacobi what his experience was as regards the presence of albumen in the urine. He stated that he had not found it in the cases he had examined.

Dr. Jacobi replied, that this albuminuria had been pointed out as a prominent symptom by Dr. See, of Paris, but that he had found it present in only about 25 per cent. of the cases he had examined.—*Am. Med. Monthly.*

Dr. Filhot considers the carbonization of organic matter, and its subsequent treatment by nitric acid, an excellent method for the detection of arsenic. He says that Orfila had repeatedly approved of it.

London Lancet.

[From the Boston Medical Journal.]

THE EFFECT OF CLIMATE UPON TUBERCULAR DISEASE.

This question has been so much discussed, that many, at the present time, are in great doubt about the conclusions adopted by those who have most thoroughly investigated the subject. On this account, it seems desirable to give a concise summary of a review of several works, in the *Archives Générales* for August, 1859, by P. Garnier.

That tubercular disease of the lungs is one from which many recover, is clearly proved by the results of autopsies: but there is still much doubt concerning the best means for promoting recovery. Medicament after medicament has been tried, has been held in high favor for a time, and has gradually lost its reputation. But among the general curative means which have been resorted to, a change of climate holds the first place. Unfortunately, the study of climatology has not, generally, been prosecuted with that scientific accuracy which its importance demands. Some specific climate has been sought, suitable for all phthisical persons, without regard to the great difference in cases. An effort should be made to place the sick in an atmosphere which bears some relation to their strength and constitution, the period of the disease, and the exciting cause.

The effect of climate is noticed particularly upon the cutaneous transpiration. M. Edwards found it, in a healthy man, five or six times more abundant in a moderately dry air than in one saturated with moisture. On the contrary, it is reduced to a minimum under the opposite circumstances, as in deep and sheltered valleys. Dr. Fourcault, mindful of the close relation existing between this function and that of the lungs, has shown the influence of its variations upon chronic diseases, particularly phthisis. The latter is frequent in low, cold, moist places, and relatively rare in dry, elevated ones. It is most rare among those who lead active lives in the open air. This is shown by statistics prepared in London, Paris, Geneva, Vienna and Hamburg.

The fact that the inhabitants of certain countries are liable to phthisis should not lead us to suppose that the climate would be prejudicial to those from colder regions, for it is generally the

case that a climate warmer than that in which the patient lives is most beneficial, as the power of generating heat is less than in healthy individuals. In the French Antilles, where phthisis is very common among the native inhabitants, it is hardly known among Europeans. The same is true with regard to Senegal, Rio de Janeiro and Egypt. Still, it does not follow that cold climates are always hurtful, but on the whole a certain amount of heat is best. But it must be remembered that there is a difference in cold climates with regard to moisture. A dry, cold climate agrees with some phthisical persons, particularly those who are in the habit of exercising in the open air.

In commencing phthisis, while the strength is good, and the disease is making but slow procets, and the pulse is not accelerated, the best climate is one which is mild, dry and stimulating, where the air is moderately agitated, without too great or constant variations. Such is that of Upper Egypt, of the southeast coast of Spain, of Nice and Naples.

For lymphatic subjects, in whom the circulation is slow, simply warm and dry places are preferable. Mountainous regions are equally beneficial, when the excitement produced by them is not of too long duration.

Patients who are excitable, nervous, disposed to hæmoptysis and inflammation, and in whom the circulation and respiration are accelerated, above all those who have reached the second or third stage, are best in a warm, uniform, calm, and slightly moist climate, like that of Madeira, Pisa, Pau, Rome, Nice, Malta, or the Isle of Wight. The climate of Pau and Rome has such a sedative action that depression is caused in healthy persons as well as in the phthisical. Many of the latter who are benefited by remaining a few weeks in Rome, see their health fail and their symptoms increase if they reside there five or six months: hence a change is often desirable.

An extremely cold temperature, when uniform, appears in certain places to prevent phthisis. Thus, according to Professor Martins, this disease is infinitely more rare in Norway under the 70th parallel of latitude, than at Stockholm under the 59th parallel. In Finmark, he had not seen a single case. In America, it

is almost unknown in the northern parts of Canada. In the United States Army, the minimum of deaths from phthisis is in the northern part. In the French Army, the maximum is noticed among the soldiers in the south.

The same exemption is seen upon the elevated plateaus of the Andes, and quite a number of observers state that the disease diminishes very rapidly in proportion as the altitude of places increases.

The influence of equatorial regions upon phthisis is to render its progress much more rapid and more acute than elsewhere.

Taking into consideration the advantages of different places, it is conceded that Madeira holds the first rank.

[From the British and Foreign Medico-Chirurgical Review.]

ON THE TREATMENT OF DIARRHŒA OCCASIONED BY DENTITION.

By Dr. Eisenmann, of Wurzburg. (Bull. Gén. de Thérap.)

At the period of their first dentition, children are often attacked with diarrhœa, not generally of a severe character, but sometimes, and especially when the dentition coincides with weaning, the vacuations are copious, and the diarrhœa passes into the chronic form. An extraordinary emaciation then ensues, and sometimes nervous symptoms of various kinds supervene, especially the *hydrocephaloid* disease, which has been mistaken for acute hydrocephalus. When it has passed into the chronic stage, this diarrhœa is very frequently fatal, the patients dying of marasmus. For this affection, Dr. Eisenmann has successfully employed a combination of sulphate of copper with opium. He prescribed it first in the case of a child whom he attended in the year 1838. The diarrhœa had not at first excited much attention, but the evacuations soon become serous, lasted three months, and reduced the child to a mere skeleton. She was in a continual state of *coma vigil*, the pulse was very frequent and scarcely to be felt, the appetite was lost, and the thirst was inextinguishable. The sulphate of copper and opium, both in very small doses, were prescribed in powders to be taken three times

a-day. In the first four days of the treatment there was already a perceptible amendment, the evacuations became less frequent and less serous, and three days after the patient began to be convalescent. The diarrhœa ceased, the appetite returned, and the digestion was reëstablished, the strength and the normal size of the body reäppeared, and four weeks afterwards the cure was complete. Dr. Eisenmann was afterwards called upon to treat other cases of the same kind, and, by employing the same remedies, similar favorable results were obtained. In the year 1840, he had occasion to observe a fourth case of this kind of diarrhœa. The patient presented the same symptoms as those above described, with this difference, that there was vomiting in addition, and that the disease had lasted only a fortnight. The emaciation was, however, already very considerable: the abdomen was somewhat tumefied and painful on pressure. The appetite was bad, and the thirst urgent; the respiration was accelerated, and there were mucous rhonchi, without any symptom of pulmonary lesion, the face was pale and a little puffy, and the looks expressed apathy. Sulphate of copper and opium, in very small doses, were prescribed, and twelve doses brought about convalescence in four days.

Surgery.

THE EXTIRPATION OF THE HUMAN MILT.

BY O. MARTINI, M.D., OF DRESDEN.

The lamentable inclination manifested by some to transfer upon the diseased human body any operation which proves not absolutely fatal in a healthy dog, cat, hog or frog, cannot be considered a therapeutical aberration only, but deserves the strongest disapproval, as a murderous attempt, if gratified, where there is in reality no indication for it, and no excuse but the desire of certain men to raise themselves in that way to a certain elevated position in surgery. We know occurrences of that kind, which we do not hesitate to denounce as unreasonable and un-

necessary injuries on the part of the physician, instead of being "a scrupulous compliance with his duty." Until it is determined whether splenotomy is entitled to any claim in surgery or not, we would remind the men referred to, of the following words, expressed by Sydenham: "Ægrorum nemo a me alias tractatus est, quam egome tractari cuperem si mihi ex iisdem morbis ægrotare contingeret."

On the other hand, our thanks are due to Gustav Simon, of Darmstadt, for the great trouble he has taken to expose, in a scientific manner, the true merits of the enthusiastically proposed new operaion, extirpating the milt. In speaking of this operation, we cannot but follow the author in the arrangement presented in his work: "Die Extirpation der Miltz am Menschen nach dem jetzigen Standpuncte der Wissenschaft beurtheilt." (The extirpation of the human milt considered from the present point of view of science). Giessen, 1857.

I. Extirpation of chronic splenetic tumors, the organ itself being in its normal place.

Ovariotomy is the only operation that may be compared with such a removal of the spleen. Kuechler (Extirpation eines' miltz tumors, mit wissenshafthicher Beleuchtung der Frage: Darmstadt, 1858) speaks of ovariotomy as "having obtained and maintaining its place in surgery;" but it is objected to by Dieffenbach, Scanzoni and Velpeau, on account of the danger unavoidably connected with it. Sixty-one ovariotomies performed in Germany resulted in a mortality of more than seventy-two per cent., while the rest of those operated upon escaped without serious damages, and only about nineteen per cent. of them derived really a benefit from the operation.

Three cases where extirpation was resorted to for the removal of chronic tumors in the spleen are on record.

Zaccarelli operated, in 1549, the wife of a Greek centurion, suffering from oppilation of the milt. That organ protruded through the wound made in the abdomen, was separated from its connection, and taken out. The patient recovered in twenty-four days. But the correctness of this case is doubted by almost all eminent surgical writers.

The second operation dates back only to 1826. The patient, a woman, at the age of twenty-two, laboring under cirrhosis of the liver and ascites, had acquired a swelling of the milt, from suppression of perspiration and menses, irascibility, &c. Tormenting pains in the tumor, and the threatening dissolution, induced Dr. Quittenbaum to undertake the removal of the spleen. Carrying the knife along the linea alba to an extent of ten inches, he severed the ligamentum gastro-lineale, ligatured the vessels without regard to the nerves, and separated the diseased organ, which weighed five pounds. Six hours afterwards the patient died from exhaustion. We cannot approve of the operation in this instance, the hopeless condition of the patient having been evident to every one.

The third case is the one related by Kuechler, in the work cited before. The tumor was fourteen inches in length, by seven in width, weighed three pounds, and had existed for fourteen years. The patient died two hours after the operation, from effusion of blood into the abdominal cavity. The cut was made along the exterior margin of the left rectus abdominis, in a length of four inches. Seven arteries were tied, care being taken not to include the nerves. There was no urgent symptom demanding the operation, the general condition of the man appearing scarcely damaged. The patient had been under observation only five days. No explanation is given of the origin and the development of the disease; nothing stated about the treatment he had undergone through so many years, not even a microscopial analysis of the blood mentioned. The pretended cause of death may be justly doubted; the facts given rather indicate the operation itself as the cause of it.

This evidence, although quite insufficient for a final decision, allows the assumption that the extirpation of chronic splenetic tumors is always highly dangerous, if not absolutely fatal. In this regard it is at least just as dangerous to life as ovariotomy.

Kuechler denies such a degree of lethality, and ascribes to the whole operation a lesser importance, while he claims a greater facility in the performance, there being, in his judgment, scarcely any of the difficulties encountered in ovariotomy, the spleen offer-

ing itself as soon as soon as the integuments are separated, and filling the wound, preventing any prolapse of the bowels, as well as the entering of air or blood. This, however, can never be prevented with certainty. A far greater wound is required: unnatural connections may interfere with the extirpation. The numerous successful operations on animals prove nothing, thousands of hogs being deprived of their ovaries without serious consequence, and still the same operation proving disastrous in by far the greatest number of instances when performed on a human being. Taking the question physiologically, Kuechler feels rather encouraged from the limited knowledge we have of the functions of the milt, and the small importance consequently attached to it. But Simon thinks the sudden abolition of that function would be more likely to increase the danger already connected with the operation, not to speak of the undecided question, if life and health can be maintained without that function. Even animals pass easier through the extirpation of the ovaries than that of the spleen, and it cannot be overlooked, that the human milt surpasses that of all the animals so far experimented on, in its proportions to the other organs of the body. Besides this, a morbidly affected milt presents other conditions by its increased size, its multiplied vessels, adhesions, &c. It can never be proved to have ceased its function, even after protracted diseases. The facts established by the removal of the spleen in healthy persons, accidentally wounded, would perhaps justify a similar operation, when an imminent danger (from gangrene, effusion of blood, etc.) can be averted thereby. It is quite another case with a chronic tumor, there being no immediate danger, and the utility of the most successful operations remaining very questionable, under the present amount of our knowledge.

The only conditions that would admit the extirpation of a chronic milt tumor are thus enumerated by Simon: No complication with disease in other organs, or in the blood; undoubted correctness of the diagnosis; immediate danger of life. But as these conditions are never existing when the spleen is morbidly increased in size, the operation is objectionable under all circumstances, except, perhaps, one: it might be justifiable when a

chronic affection, before it is considerably developed, and before it has undermined the strength of the patient to any degree, turns, by some accident, into an acute disease (rupture, for instance, or incarceration), so as to put life into jeopardy, and even then provided that there is no doubt in regard to the diagnosis, and the patient is able to bear the operation.

II. Extirpation of the milt, not hypertrophied but prolapsed, in consequence of penetrating wounds in its neighborhood.

Seventeen cases of this kind are known since 1581, the organ being either totally removed, or only a part of it. Not one of these cases ended fatally, rendering it highly probable that the spleen may be extirpated in a healthy person without impairing the life and health for at least some time. But notwithstanding this favorable conclusion, there is another indication to be minded. The spleen falling through an abdominal wound, ought to be replaced, if not morbidly altered or otherwise spoiled. A hemorrhage from the milt itself, which cannot be stopped, suppuration or gangrene of the same would certainly demand the extirpation, which might even be attempted, when, under similar circumstances, the organ is retained in the abdominal cavity.

III. Extirpation of the movable spleen (or wandering milt, as the Germans call it).

This abnormity has not been noticed until a few years ago, and the observations are not very numerous, amounting in all to about fifteen. It may be congenital or acquired. Sometimes the organ is not changed, although it is generally increased in size, when dislocated.

Kuechenmeister (Letter to Prof. Dietl, "Wien Medic. Wochenschr." 27, 1856) proposes to extirpate a wandering, movable spleen, if the trouble it causes is not removed by quinine. Certainly a correct diagnosis is a *conditio sine qua non*.

According to Simon, the symptoms dependent on such a state are more distressing, and at the same time more distinct, than those connected with a tumor in the normal place. The experience of Dietl shows the inefficacy of any therapeutical remedies. Furthermore, there is no great danger in the operation, it being admissible before the tumor acquires any considerable size;

hemorrhage can be easier prevented, ligatures applied more securely.

Against such an operation, however, there are many and strong objections. We cannot but consider the extirpation, even of a wandering spleen, much more injurious to life than ovariotomy. Then, it is not yet proved that the removal of the spleen, under the most promising circumstances, does not result ultimately in heavy damages to health and life. Complications which would contra-indicate the operation, seldom escapes the most scrutinous examinations. All symptoms referring directly to the tumor (such as heaviness in the lower parts of the body, anæsthesis and paralysis of the lower extremities) imply no danger to life. Some relief at least can always be afforded by a proper attendance to the case. Lastly, the progress and the consequences of the affection are too little known to allow any estimate of its perilousness.

Balancing these considerations, we find the operation inadmissible, with the single exception adduced for swellings of the spleen in its normal place.

Dr. Piguacca, who has a splendid occasion to study the tumors of the milt, in the numerous cases occurring in the hospitals of Pavia, never saw (Ann. univ. Febr., Marzo, 1857,) any "manifest incommodation" from a wandering spleen. He considers the extirpation as an unwarranted procedure, as long as there are no considerable disturbances in the natural action of other organs; and this was never the case in the many patients that came under his observation.

Prof. Adelmann (Deutsche Klin., 17, 18, 1856,) has taken sides with Dr. Kuechler, defending the bold course adopted by the latter. Dr. Gurtt (Deutsche Klin., 34, 1858,) and others, acknowledge the impregnable position of Simon, whose views appear scientifically correct.—*Schmidt's Jahrb.*

A Small Baby.—A living child, weighing two pounds and nine ounces, the New Orleans Charity Hospital Gazette says, was lately born at full term, at an infirmary in that city.

ARTERIES SECURED BY ACUPUNCTURE.

BY J. M. CARNOCHAN, M.D.

I HAVE recently read, with much interest, the article of Professor Simpson, of Edinburgh, upon acupressure as a means of arresting surgical hemorrhage. On Saturday last, I had an opportunity of applying this method to the arteries of a bleeding stump, after amputation, and, I am gratified to add, with a very satisfactory result. The case was one requiring amputation of the right foot, at the tarso-metatarsal line of articulation. The operation was performed by making a semi-lunar flap on the dorsal aspect of the foot, a little in front of the tarsus. The flap was then dissected backward, and the dorsal and plantar articular ligaments, between the tarsal bones and the metatarsus, severed completely. The foot being held horizontally, the narrow knife was slipped under the tuberosities of the first and fifth metatarsal bones, and carried forward, grazing the lower surface of the metatarsal bones, so as to make a flap of sufficient extent to cover the exposed stump. The anterior tibial, external plantar, and internal plantar arteries bled freely, and three smaller vessels, also, afforded blood enough to require their obstruction. Regarding this as a favorable opportunity to test the effect of acupressure in amputation, to arrest the bleeding from the anterior tibial, I took a steel shawl-pin, with a metallic head, and about four inches long, and passed the point slantingly to the depth of half an inch into the tissues, at about an inch and a half from the course of the artery on the side nearest. The point having passed onward, was made to emerge about a line from the artery. The pin was next directed over the trunk of the vessel, about a quarter of an inch from the bleeding orifice, and again dipped into the tissues on the other side of the vessel, about a line distant from it. The pin was then still pushed through the tissues for about an inch and a half, and again made to emerge onward for an inch. The compression on the artery was complete, and it ceased to bleed. The external plantar artery was next treated in the same manner, and with a similar result, as well as the internal plantar and the other vessels which would

have required the ligature. In these last-mentioned arteries, not having at hand acupressure needles of suitable length, I resorted to short suture-pins, tying a piece of thread to the head of each, in order that they could be pulled away at the proper time. In securing the anterior tibial artery, I did not think it necessary to pass the pin through the integuments, as Professor Simpson recommends. The flaps were now brought together by points of suture, and the long pin and the threads attached to the shorter ones left between the lips of the line of union.

The pins were removed on the seventh day. Since the operation, the patient has been most comfortable, and without the slightest evidence of secondary hemorrhage.

Judging from the results obtained in the cases just related, I have no doubt that acupressure will become a distinct and established method for arresting hemorrhage in operations; and that, although it may not supersede the use of the ligature, it will, in many instances, supplant its use, as being more simple and equally effective, and as less likely to interrupt the primary union of wounds.

The celebrated Edinburgh Professor merits the thanks of the profession for formulizing "acupressure" into a distinct method.

IMMEDIATE CURE FOR IN-GROWING NAIL.

Dr. N. Gilman recommends (Boston Med. and Surg. Journal, Dec. 29, 1859) cauterizing the part with hot tallow as a speedy mode of curing this troublesome affection.

"The patient," he says, "on whom he first tried this plan was a young lady who had been unable to put on a shoe for several months, and decidedly the worst case that I have ever seen. The disease had been of long standing. The edge of the nail was deeply undermined, the granulations formed a high ridge, partly covered with skin, and pus constantly oozed from the root of the nail. The whole toe was swollen, and extremely tender and painful. My mode of proceeding was this: I put a very small piece of tallow in a spoon, and heated it over a lamp

till it became very hot, and dropped two or three drops between the nail and the granulations. The effect was almost magical. Pain and tenderness were at once relieved, and in a few days the granulations were all gone, the diseased parts dry and destitute of feeling, and the edge of the nail exposed so as to admit of being pared away without any inconvenience. The cure was complete, and the trouble never returned.

"I have tried this plan repeatedly since, with the same satisfactory results. The operation causes but little, if any pain, if the tallow is properly heated. A repetition might in some cases be necessary, although I have never met with a case that did not yield to one application. The liquid cautery insinuates itself into every interstice, under the nail, along the fistula into the ulcer at the matrix of the nail, accomplishing in one minute, without pain, all that can be effected by the painful application of the nitrate of silver for several weeks. Let this simple plan be tried before resorting to the barbarous plan of pulling out the nail, or any other mode of torture heretofore invented."

Deviation of Naval Septum from a Blow.—M. Demarquay has adopted a new method for the relief of deviation of the nasal septum resulting from a blow of the fist. He made along the median line of the nose an incision, which, commencing at the back of this organ, ended upon the upper lip. The first stage of the operation separated the two lateral cartilages of the nose, and conducted to the median cartilage. This accomplished, M. Demarquay dissected to the left the mucous covering of the cartilage which filled the nostril; and when all the projecting part of this cartilage was thus exposed, he raised it, cutting from behind forward all that obstructed the nostril and hindered the respiration from being accomplished. This done, he reunited, by points of suture, the divided lobule of the nose, and the affection was cured perfectly. The nose was straightened, and the respiration reëstablished upon the left as well as the right side. Reunion was accomplished by the first intention; and when the young man left the hospital there remained no more trace, either of the primary accident, or of the operation to which it had given place.

Valsalva's Treatment of Internal Aneurism.—Instead of profuse and repeated bleedings and rigid starvation, Dr. Brady, of Dublin, proves that Valsalva's treatment consisted in not more than one or two bleedings at the outset, and subsequently complete rest in bed, enemata, and abstinence from wine, "with so much food and drink, weighed and measured, as would be sufficient to support life, and this not divided into two portions daily, but into three or even four, so that entering the bloodvessels in small quantities, it should not, in the slightest degree, distend them." So much for the accuracy of medical history.

N.Y. Medical Press.

Midwifery.

CLINICAL LECTURES ON THE DISEASES OF WOMEN.

By J. Y. Simpson, F. R. S. E.; Professor of Medicine and Midwifery in the University of Edinburgh.

LECTURE XXVI.—ON OVARIAN DROPSY—ITS SURGICAL TREATMENT.

Gentlemen: Great diversities of opinion have prevailed with respect to the proper principles on which the surgical treatment of cystic or dropsical tumors of the ovary ought to be conducted: many methods of surgical treatment have been proposed; and some authorities have doubted the propriety of all surgical interference whatever. It would be almost an endless, and, withal, a very profitless task, were I to dwell upon all the details of the different modes of surgical practice that have been resorted to in the treatment of ovarian dropsies. You will be better able to undertake the management of a case when it comes under your own observation, if I now endeavor to point out and illustrate the general principles which must be followed in the treatment of the malady. But let me premise a hope that, in the course of a few years—especially as the attention of the profession is now strongly directed to the subject—we may yet see all the means of its surgical treatment still more systematized and reduced to order than they have ever yet been, for then only shall

we be able duly to appreciate each, and to know in what case to adopt or in what to reject it.

Of all the surgical means, then, by which it has been attempted to reduce, and keep reduced, an ovarian tumor, perhaps the simplest is—

I. *The application of Pressure.*—Mr. Benjamin Bell and Doctor Hamilton, of this city, tried, long ago, to dispel ovarian tumors by means of pressure; and in later years this plan of treatment has been revived by Mr. Baker Brown, who has carried it out much more perseveringly and systematically, and, it is reported, with better success. On what principle, some of you may ask, could we ever hope to arrest the growth or bring about the resorption of cystic ovarian tumors by the application of pressure? I have already tried to show you that such tumors tend to develop themselves chiefly in the direction in which they meet with least resistance to their increase and growth, and it was supposed that, by presenting an adequate obstacle to the growth of the tumor towards the abdominal cavity as persistent and effectual as that which it naturally meets with in the hard-walled cavity of the pelvis, the further progress of the growth might be stayed, and, perhaps, even a certain degree of diminution produced in the size of the tumor. The same principle of treatment has been applied to other forms of morbid growths also, by Recamier, Arnott, Young, and others. It has been attempted in this way, for example, to check the growth of cancerous tumors, and to bring about their destruction by subjecting them to such a degree of pressure as would hinder the further development of the individual component cells of the compressed mass, or lead to their speedy dissolution and decay. This kind of treatment was carried out long and perseveringly, and in many different ways; but all hope of curing cancer in this way has now, I believe, been given up; and, I believe, that for the cure of ovarian tumors, too, such treatment is almost equally hopeless. Compression of ovarian tumors was effected by means of books, metallic presses, or folds of lint and linen cloth, laced and tied over the abdomen by all kinds of bandages. The application of the compressing agent was sometimes preceded by a preliminary

tapping, with the view of preventing a reäccumulation of the fluid in the interior of an evacuated cyst, and with some hope, also, of producing the cohesion of its collapsed walls. I have known cases where this treatment has been tried, and have been told by the patients subjected to it that the amount of pain and distress produced by it was almost intolerable. If you inquire with what degree of success this plan of treatment has been attended, I think you will find it but very slight; and I believe the very few and isolated cases of cure that have been reported as probably or possibly resulting from it will hardly induce you to submit any of your patients to such an irksome and painful process.

II. *Paracentesis.*—The oldest practice, and that which is still most frequently had recourse to in the treatment of cystic tumors of the ovary, is the removal of the fluid from the largest and most prominent cyst or cysts by tapping them with a trocar and canula, or even by means of a simple knife or lancet. Various questions at once present themselves in connection with this mode of treatment. Let us see, first of all, under what circumstances we ought to puncture an ovarian cyst—or, in other words, let us study—

1. *The Cases requiring the Operation.*—In Britain the operation of paracentesis abdominis is, as a rule, almost never had recourse to till the ovarian tumor has attained such a size that it compresses injuriously the abdominal organs, and interferes with their function, or even impedes the action of the diaphragm, and renders respiration difficult, so that the patient is subject to much discomfort and suffering, her health, perhaps, already damaged and impaired, and, it may be, even her life itself is endangered by the distension to which he is subjected. Some writers have attempted to determine and define in what particular cases and conditions the operation should be performed; but, whatever rules you may find laid down regarding it, I repeat, that when you come to practice, you will usually find that the degree of distress which the patient is suffering, or the degree of danger to which her health or life is exposed, are the indications that guide and impel you to the performance of the operations.

Having decided as to the necessity of the operation in any particular case, you have next to determine where the tapping should be made, or,

2. *The Place of Puncture.*—The puncturing instrument is usually introduced in the linea alba a few inches below the umbilicus, at about the upper third of the distance between the umbilicus and the symphisis pubis, because in most cases the cyst is in this locality sufficiently prominent to be easily pierced, and there are no vessels of any consequence in the abdominal wall which are likely to be a source of hemorrhage, if they should happen to be injured by the trocar or lancet. I don't very well know for what reason, some surgeons have proposed to evacuate ovarian cysts by puncturing them through the bladder. I can easily see that there might be a good deal of danger attendant on such a proceeding, but what advantage it could have over other and safer forms of the operation, I do not understand. I have sometimes tapped ovarian dropsies at other points of the abdominal walls, as in the middle line above the umbilicus, or in one of the lineæ semiculares below it; and we are obliged to select these or other spots in those exceptional cases where the cystic mass is too dense at the usual site for abdominal paracentesis, or where the trocar and canula have failed to draw off any sufficient amount of fluid when introduced in the usual position of the linea alba. If the subcutaneous veins are much increased in size—as they often are vicariously in ovarian dropsies which are large enough to compress the vena cava inferior—be very careful and choose for the insertion of your trocar a spot which presents no enlarged veins beneath the skin. But in tapping ovarian dropsies the abdominal parieties have not always been selected as the seat of puncture: sometimes they have been tapped through the vagina and the bladder, and I have read of the proposition of tapping also ovarian cysts through the rectum. The primary objection to performing the operation, as a common rule, through any such dependent localities, is simply this—the denser portion of multilocular ovarian tumors is, as I have already explained to you, almost always placed inferiorily in the cavity or at the brim of the pelvis, and we cannot usually, easily, or at

all reach the larger cyst or cysts of the tumor by entering the canula from below. In some exceptional cases, however, we can readily enough empty the tumor in this way; and if we can, there is no valid objection to performing paracentesis, as Nonat, Naumann, Watson, Arnott, Ogden, and others have done, by puncturing the dropsical collection through the roof of the vagina.

3. *Results of the Operation.*—If you ask, What results are to be expected from this operation, and with what hope of cure may it be undertaken? I must at once tell you that the hopes of an ultimate and complete cure by means of it are very faint and slight indeed, and that it is usually had recourse to only as a means of temporary palliation. It has happened comparatively but very rarely that the simple tapping of a cystic ovarian tumor has been followed by a final cure of the disease. In ninety-nine cases out of a hundred, where the patient survives the first tapping, the operation requires to be repeated again and again, and every time it is had recourse to the interval, as a general law, becomes shorter and shorter between every two successive tappings. Sometimes women have survived for many years a series of tappings, and enormous quantities of fluid have, in such cases, been drawn off from the cysts, as the following table shows:

TABLE OF OVARIAN TAPPINGS, ETC.

CASES.	NUMBER OF TAPPINGS, ETC.	QUANTITY OF CONTENTS.
Lady Paget (Dr. Mead)....	.... 67 in 5 years.....	240 gallons.
Mr. Ford's case.....	.... 49	350 "
Ramsbotham's.....	....129 in 8 years.....	461 "
Morand.....	.... in 10 months ...	427 "
Martineau of Norwich	.... 80 in 25 years.....	729 "

The table shows you, I repeat, the frequency with which the operation of tapping has been repeated in a few of the more rare and remarkable cases that have been put on record, and the large quantity of fluid that has been abstracted in each case in the course of these oft-repeated evacuations. Sometimes you may possibly meet with a case where the patient does not succumb until after the lapse of many years, and after the operation has

been very frequently repeated. But that is certainly not all the history of the ordinary run of instances of ovarian dropsy, for the patient usually dies after the operation has been only a few times performed. Let us ask, then,

Is Tapping a dangerous operation?—I believe it is by no means free from danger, more especially when performed for the first time. I have just told you that in some instances the operation has been repeated a great many times upon the same woman with the most perfect impunity, and the observation of cases of this kind where patients have been thus frequently tapped, may have led to the conviction impressed on many professional minds that the operation is as safe as it is simple and easy of performance. But that the operation is not so simple and harmless as it appears, some of you have had an opportunity of seeing in the case of a patient who died in the hospital in the early part of the year. I doubt not some of you will be able to call to memory the patient I refer to. She was suffering from a large encysted tumor of the ovary, the great bulk of the disease being produced by the excessive development of a single cyst, which I tapped with the ordinary trocar and canula. The operation must have appeared to you a very simple one; there was no kind of complication or peculiarity about it; and certainly we were but little apprehensive of danger at the time; but, in a day or two, inflammation sprang up in the interior of the evacuated cyst, and the patient died of a form of surgical fever. About the time when that fatal case occurred in the hospital, I saw, along with Dr. Goldstream, another instance of the disease in the person of a young unmarried lady, who had, when in England, consulted Dr. Clay as to the propriety of ovariotomy. Before, however, finally determining on that point, or on the injection of iodine, or other special measure, we agreed to make one preliminary tapping, to see what might be the nature of the contained fluid—whether there were any adhesions, and what, in short, were the exact characters, relations and conditions of the tumor, as far as they could be ascertained, after the evacuation of the principal cyst. A quantity of pale, bland-looking fluid escaped, and there was nothing noticed at the time in connection with the

case to make us apprehensive of the result. But, as in the other case, the lining membrane of the cyst inflamed, and the patient died. Two cases of this kind, occurring so nearly at the same time, serve to remind us very forcibly of what we so readily forget in the midst of the ordinary run of successful—or, at least, innocuous—cases of the operation, viz., that simple tapping of a cystic ovarian tumor may sometimes be fatal. In these two instances, the patients were both operated on for the first time; and I mention this circumstance in order to impress it more emphatically upon you, that the operation is chiefly dangerous when the cyst is for the first time evacuated. When once a patient has been subjected to the operation successfully, or, at least, without undergoing any special hazard, she is comparatively safe, and the operation may be repeated as often as the urgency of her symptoms require it without the same degree of fear of a fatal issue. When a tapping is performed for the second, third or fourth time, it seldom, by itself, proves fatal; for when the patient survives the first operation, it is not until she has become exhausted by the excessive accumulations and repeated evacuations of the fluid that she finally sinks from gradual exhaustion and irritative fever which succeeds the series of operations. Thus, I have before me a preparation of encysted dropsy of the ovary, taken a short time ago from the body of a patient who died of that disease. The tumor had been growing for about three years, and in the month of July, of last year, it had attained such a size, and interfered so much with the patient's health, that I was obliged to tap her then, for the first time, to relieve her of her urgent symptoms. She got over the operation remarkably well, and enjoyed tolerable health for three or four months; but in the end of November the tapping had to be repeated, because of the great reäccumulation of the fluid and the recurrence of the symptoms. She thus again procured a temporary relief, but now of shorter duration, for in two months the tapping had to be again performed. Her recovery on this occasion was not so perfect, and when, in a few weeks afterwards, the fluid was again drawn off, it was found to be mixed with pus. From this time her health and strength rapidly declined; the paracentesis

had again to be repeated three different times, and after each renewal of the operation she was found weaker than before, till at last she sank and died, in rather less than a year from the time the operation had been first performed. Such a history is the common sad history of many a case of ovarian dropsy and ovarian tapping; though there are many where the progress of the disease is less rapid, and where the evacuation of the fluid requires to be effected much more frequently ere the patient finally succumbs. But all of them alike show that when the first tapping has been safely and successfully got over, the operation may usually be repeated often with less immediate danger. Why is it—you may ask—that the operation is so much more dangerous when it is performed for the first time? It is usually supposed, and it has often been said, that tapping an ovarian cyst is a very simple operation; but in first cases, at least, it is really not so. After the operation has been once performed, you may often expect some adhesion to have taken place between the surface of the tumor and the abdominal parieties, so that the puncturing trocar most probably only penetrates these fibrous masses in future operations; while in first tappings, or in cases where no adhesions have been formed between the tumor and the abdominal wall, the peritoneum is wounded at two points, in one or both of which inflammation may readily be set up, and whence it would speedily spread over a large extent of surface. The fluid escapes usually to some degree into the peritoneum, and all may go on safely enough if this fluid be healthy in character, and unmixed with inflammatory products. But in first cases, with the operation usually postponed as long as possible, you will not unfrequently find that the fluid that flows out is mingled with inflammatory products; and should any of this deteriorated and morbific matter escape past the canula, or through the unclosed aperture of the cyst-wall into the cavity of the peritoneum, it would be sure to light up a degree of inflammation there which might soon prove fatal. We have not very many statistics relating to this subject, but I show you here a table, from which you can learn both how frequently ovarian tapping proves fatal after its first performance, and how very speedily, in

the majority of cases, the disease hurries on to a fatal termination after the practice of tapping the cyst is once begun. The table is drawn from an essay by Dr. Fock, published at Berlin in 1856. Several of the patients were tapped many times. The table shows merely the

Date of Death of the Patients after First Tapping in 132 Cases of Ovarian Dropsy.

25	died within	some hours or a few days.
24	"	six months.
22	"	the first year.
21	"	the second year.
11	"	the third year.
29	only were alive at end of last date.	
132		

The mortality from first tappings, or, in other words, the proportion of the 132 patients that died within a few hours, or a few days at most, after the operation was performed for the first time, was 1 in every 6. Professor Kiwisch, of Wurzburg, lost 9 patients out of 64, or about 1 in every 7, within twenty-four hours after their first tapping for ovarian dropsy. Several years ago Mr. Southam, of Manchester, published a table, from different sources, of the results of twenty cases of ovarian dropsy treated by paracentesis. Of these 20, 4 died within a few hours after the first tapping; 3 within the first month; 14, in all, within nine months. Of the remaining 6, 2 died in eighteen months, and four lived from periods varying from four to nine years. Dr. Meigs, of Philadelphia, speaks of his impression being that, among the first tappings of ovarian dropsy which he has witnessed during his long and useful professional life, nearly one-half have been speedily followed by the death of the subject from peritonitis, developed soon after the performance of the operation. This mortality is certainly much larger than the average.

But I have said enough to make you cautious and careful in regard to your prognosis in reference to tapping an ovary for the first time. In short, all experience and all statistical records go to show that the operation of tapping for cystic disease of the ovary is not always a safe operation, that it is not usually a satisfactory one, and that it very seldom proves successful. It

is very rarely, indeed, that it ever produces a cure; at the best it is usually only a temporary palliative, relieving the patient from symptoms very distressing and immediately dangerous: but, after a longer or shorter period, the symptoms of distress and danger, thus abated and averted, are almost sure to return, and call for a renewal of the operation; and so the case goes on, with intervals of relief following the repeatedly-performed operation, but growing always shorter and shorter, till finally the patient gets worn out and dies.

I shall continue my remarks on the surgical treatment of ovarian disease in my next lecture.

Another Remedy for Ascarides.—The American Medical Monthly says, Dr. Compénat has got a cure for ascarides, which has never failed in his hands. It is a simple injection of water, containing five, ten, fifteen or twenty drops of sulphuric ether, according to the age of the individual, and repeated more or less frequently, according to the number of the animals present.

Syphilitic Eruptions.—Mr. Simon is accustomed to employ, in the wards of St. Thomas's, for the cure of tubercular syphilitic eruptions, the applications, once daily, of a small portion of unguent. hydr. mite to every spot, in addition to the ordinary constitutional measures.

Strabismus.—Du Bois Reymond (Muller's Archiv. and Bull. Gén. de Thér.) recommends the *stereoscope* for the cure of squinting. The principle is the same as in the case of the prismatic spectacles, advised by Mr. Spencer Wells.

Ptyalism.—Dr. Erpenbeck (Schmidt's Jahrb) relates a case of severe mercurial salivation arrested by the internal use of belladonna. The salivation returned when the remedy was discontinued, and was again checked by it.

Editorials and Miscellaneous.

MEETING OF THE STATE SOCIETY.

It is well known to our readers that the State Society assembles in Washington on the 25th of April next, and that important business will be brought before it.

The time was when an earnest appeal on our part might have been necessary to secure a large attendance on that occasion; but, fortunately, a brighter day has dawned upon the fortunes of this Association, and its annual meetings are now the occasion of such grand reunions of the physicians of North Carolina, as must delight the heart of every man interested in the advancement of the Profession. From the mountains to the sea, from the Virginia line to the Palmetto State,—amid the umbrageous swamps of the lowlands and the rocky haunts of the mountains,—wherever physicians have been led in the pursuit of their vocation, a deep and abiding interest is now felt in all that pertains to the State Medical Society. The value of its services in the cause of medical reform is fully appreciated: the importance of those great objects for which it was founded is properly recognized and duly estimated: the noble stand it has taken in defense of professional etiquette has the earnest sympathy and cordial support of all save the miserable hirelings who cannot discriminate between venality and virtue, and whose daily lives of chicanery and prostitution dishonor them as much as they disgrace humanity: whilst its benignant instrumentality in cultivating a spirit of fraternity among the physicians of the State has embalmed it in the affections of those whose hearts have expanded under the genial glow of its festive scenes, and whose lives have been lighted up by the cheering beams of its enduring friendships.

The truth is, the State Society has been a success—as complete a triumph as its most ardent friend could expect,—and we can but rejoice over the fact. Commencing from a "small beginning," and under most disadvantageous circumstances, it struggled in obscurity for years, harassed by the lukewarmness

of friends, and embarrassed by the evil vaticinations of that misanthropic class of philosophers who take a fiendish delight in evil predictions, but has finally become one of the chartered institutions of the State, honored within the limits of Carolina, and respected throughout the Union.

For these reasons, we expect to meet a large number of our professional brethren at Washington; and, between shaking old friends by the hand, forming new ones, enjoying the hospitalities of the place, and receiving the most substantial encouragement to the Journal, we anticipate a glorious time of it altogether.

But though the Society has accomplished much, it has still more work to do before the completion of its labors. But few Reports have been presented to the Society, whilst such as have been offered are generally either controversial in their character, or confined to a description of isolated cases or unusual epidemics. Nothing would conduce more to the interests of its annual meetings, or prove of greater advantage to the cause of science, than an accurate *Medical Geography of the State;* and the Society, by judicious management, has it in its power to secure this desideratum, at no distant day. Of course, a work of this character cannot be immediately effected, but the requisite materials can be gradually collected and arranged by committees properly selected, and impressed with the importance of the work, until the whole of Carolina is accurately mapped out according to its medical topography. Surely the vast importance of this work will commend itself to the attention of the Society, and induce the physicians of the State to coöperate heartily and promptly in its execution.

Again, a good Registry Law is imperatively demanded by the interests both of the profession and the laity, and the State Society is the appropriate organ through which to secure its establishment. This is not the place to demonstrate the advantages to be derived from a proper registration of "marriages, births and deaths," together with the preservation of such important facts as may present themselves in this regard; but it will be sufficient to say, that the existence of such a law is esteemed a matter of the first importance by the scientific men of the times, and

that the most enlightened communities have incorporated it into their statute books.

A determined effort on the part of the Society might eventuate in the passage of such a law, and we can but express the hope that some decided action will be taken in the premises.

We might mention other subjects in which the Society should interest itself, such as "a State Medical College," "Medical Orphanage," "the Sale of Poisons," &c., but space will not permit a proper discussion of their merits, and we shall reserve the consideration of them for some future occasion.

The question of amending the Medical Bill will, of course, be discussed—fully and impartially, we trust—as we feel well assured that the Society will then determine to alter it in no material point for the present.

As far as the Journal is concerned, we shall have the pleasure of reporting it in such a prosperous condition as will give satisfaction to its friends, and induce the Society to continue the patronage so liberally bestowed upon it, as that is necessary to the very existence of the work, unless its Editor is willing "to burn the midnight lamp" at his own expense. But, as our absence from the last meeting of the Society did not prevent the expression of a favorable opinion of the manner in which the Journal had been conducted, we sincerely hope that our presence in Washington will not close the mouth of any man who may be disposed to censure us editorially or otherwise. Then the Journal was in its infancy, and dared not attempt to stand alone; now, it has "reached man's estate," and "can answer for itself," so,

"Lay on, Macduff,
And damn'd be him who first cries Hold!—enough!"

In conclusion, we will simply say, that from the information which has reached this office, the Washington meeting will be one of the largest, ablest and most enthusiastic that the history of the Society has known; and, from our knowledge of the citizens of that town, we are confident that all who attend will receive an old-fashioned Carolina welcome.

TURNING BY EXTERNAL MANIPULATION.

This subject has been invested with peculiar interest of late, by the controversy between Dr. Langer and the Scott County Medical Society of Iowa, to which we referred in the last number of the Journal. This notorious individual has sought to vindicate himself against the sentence of his professional peers, by quoting an immense number of authorities in support of the propriety of resorting to external manipulations in certain malpositions of the fœtus; and though it is quite evident, his whole conduct had been that of a quack, he has found friends and supporters in various sections of the country, who pretend to claim for him the enviable position of a martyr to the cause of science.

Now, without joining issue with him or his advocates as to the possibility of effecting cephalic version when attempted properly, or discussing the propriety of the action by which he was expelled from the association of his more respectable brethren, we desire to state a fact, which can be established at any time by a series of observations on women of emaciated person, in the latter months of pregnancy. The fœtus, in a large majority of cases, remains in one position but a limited period, and, in truth, makes the whole circuit of the uterus during the period of gestation,—so that the nature of the presentation is determined by the particular moment at which labor commences.

The truth of this assertion can be easily verified, not only by auscultation in ordinary cases, but by watching the movements of the fœtus when the abdominal parieties have become so attenuated as to render its various parts distinguishable.

Without defending the conclusion at which we have arrived in regard to the influence exerted by these fœtal movements in producing uterine contractions and the expulsion of its contents, we desire simply to apply this fact to the controversy in question. If such mobility attaches itself to fœtal life, where is the wisdom in attempting to rectify an unnatural position before the beginning of labor? Is it not plain, that if the child does move in the womb, in response to certain intrinsic influences pertaining to that viscus, it is capable of resuming even its original

position, or any other, in the progress of parturition? Can an "enlightened science" demand such an interference as cannot eventuate in any permanent advantage to the patient, and which subjects her to an indecent, unnatural and unnecessary exposure? Common sense gives an answer to all of these questions which completely unravels the tissue of contradictions whereon this innovation is founded, and shows that, like their "self-constituted" expounder, they are exceedingly slack-twisted.

That "external manipulation" has "turned" some heads, in these latter days, can be denied by no one who has observed what the labors of the illustrious Langer have brought forth, with the New York Journal as his accoucheur. There will doubtless be a "breech presentation," however, when the National Association meets; and we deeply regret that craniotomy *is not possible* under the circumstances, so that the Profession might be relieved of the acephaloid at once and forever.

A Mistake. — Some of the Northern Journals have stated, "the Medical College of North Carolina" invited the Southern students to secede from the Philadelphia schools. As there is no such institution in existence, the report is, of course, without foundation.

Boston Medical Journal.—Drs. Oliver and Ellis have succeeded Drs. Morland and Minot in the management of this Journal. As "De mortuis nil nisi bonum" seems to be the maxim of the Editorial fraternity, we shall be as silent as the grave about these "retiring gentlemen" (?).

Dr. F. Peyre Porcher obtained the prize of the South Carolina State Medical Society for an Essay on "the Microscope."

Lænnec died of phthisis, and Alibert of cancer. One had innoculated himself years before with tubercule and the other with cancerous matter.

Consumption is rare in the Arctic regions, in Siberia, Iceland, the Faroe Islands, the Orkneys, Shetlands and Hebrides; and in confirmation of the opinion that it decreases with the decrease of temperature, it is shown, from extensive data, that in Northern Europe it is most prevalent at the level of the sea, and that it decreases with increase of elevation to a certain point. It is uniformly more fatal in cities than in the country.

The "Docteur Noir" has been arrested in Paris, under the charge of homicide. He wrote to one of his victims, that he knew she was dying, for she had only paid 10,000 francs, but that he would cure her still for 40,000 francs. The patient died, and the doctor is in jail.

The American Medical Association meets in New Haven in June next.

A Naval Medical Board met in Philadelphia on the first of March, for the examination of assistant surgeons for promotion, and of candidates for admission.

Errata *in Dr. Howard's article, in the October number:—*

Page 11, line 23 from top, after "characters," insert, *it is not possible that.*
" 14, " 15 " for "Dr. Harrison," read *Dr. Manson.*
" 30, " 15 " for "*in* percussion," read, *on percussion.*
" 32, " 10 " for "their," read *thin.*
" 41, " 9 " for "discovers," read, *discovered.*
" 43, " 26 " for "which are," read, *which is.*

In the January number:

" 107, " 23 " for "Fante," read, *Fonte.*
" 131, " 12 " for "are found," read, *is found.*
" 134, " 15 " for "gradulated," read, *granulated.*

In this number:

" 209, " 11 " for "case," read, *care.*
" 215, " 14 " for "prodormata," read, *prodromata.*
" 218, " 1, for "*a* complicated," read, *the complicated.*
" 220, " 20 from top, for "and disappear," read, *or disappear.*
" 224, " 1 " for "Martin, Solon," read, *Martin Solon.*
" 229, " 4 from bottom, for "medical," read, *medicinal.*
" 240, " 24 from top, for "defibrinzed," read, *defibrinized.*
" 249, " 21 " for "is a," read, *in a.*
" 255, " 1, for "unison," read, *union.*

At pp. 243, '44, the reader will notice that the letters used as indices in the text, to mark the authorities referred to in the foot-notes, have been incorrectly placed, and do not correspond.

NORTH CAROLINA
MEDICAL JOURNAL.

MAY, 1860.

Original Communications and Translations.

REMOVAL OF A FIBROID TUMOR FROM THE UTERUS.

BY N. J. PITTMAN, M.D.

The subject of the following notice, Mrs. H., æt. 51, had suffered much, for a period of ten years, from a sense of pressure, and pain in the pelvic region, accompanied with uterine hemorrhage. She had been attended by several intelligent physicians, one of whom had given some special attention to surgery.

Mrs. H.'s case had been regarded as that of "inverted womb," and more than one attempt had been made to reduce it. The patient had worn pessaries of different patterns, for several years, with seeming temporary benefit.

On the 24th of May, 1857, I was requested to visit this lady, with her attending physician. I found her much reduced, from free and continued hemorrhages, having been bedridden for several months. In attempting to examine the uterus per vaginam, we found a large substance presenting external to the vulva, and completely filling up the vagina: it seemed to be firm, with patches of old ulcerations and abrasions,—fruitful sources of hemorrhage.

Proceeding with the examination, we found this growth attached within the womb (on the right side), which organ seemed to be in a healthy condition, though prolapsed from the weight of the tumor, the relaxed state of the vaginal walls, &c.

Having satisfied myself of the true nature of this growth, and the site of its attachment, I proposed its removal, though with many misgivings as to a favorable result; and, considering the extreme emaciation and exhausted condition of the patient, I could give no assurance of a cure.

After due consideration of all the attending circumstances by the patient (a very intelligent lady) and her family, an operation was determined upon. Accordingly, on the 24th of June, one month after my first examination, I introduced a large curved needle, armed with a double ligature, through the neck of the tumor (one and a half inches in diameter), within the cervix uteri, each ligature being secured on a long double silver canula; one of the medical gentlemen, in attendance, holding the labia out of the way by two instruments made for the occasion, and fashioned after M. Jobert's instrument for bringing into view vesico-vaginal fistulæ; the tumor being drawn down and securely held with a pair of strong vulsellum forceps, in the hands of another medical friend. In the commencement of our manipulations, blood flowed freely, and the patient seemed much exhausted; but, by the timely administration of brandy and black drop, we were enabled to proceed with the operation to a satisfactory conclusion; after which, having administered two grains of opium, we left our patient apparently comfortable, the operation having lasted three-fourths of an hour.

At twelve o'clock the night succeeding, patient was retching and vomiting, with great nervous exhaustion. Continued the opium, and gave freely brandy toddy and strong coffee.

25*th*.—More comfortable, though retching occasionally. Ordered pellets of ice, continuation of brandy toddy, and substituted morph. sulph. for the opium: left for home, and sent Mr. J., a pupil in my office, to watch and notice patient's condition.

26*th*.—Had a more comfortable night, having slept several hours: felt refreshed, and had no pain; slight nausea. Took some nourishment—brandy and water: discontinued the opiate, and gave quin. sulph. every fourth hour, in doses of grs. ij. Ordered generous diet, and the frequent use of a decoction of red oak bark, cold, as an injection per vaginam, alternated with sol.

sodæ chlorid. A large portion of the tumor came away during the day. No hemorrhage since the operation.

27*th.*—Was hopeful and much encouraged; having slept four hours the night before, felt quiet and refreshed. Ordered elixir vitriol, gtt. x., every sixth hour, in a glass of water, with loaf sugar: discontinued the quin.

28*th.*—Was cheerful; looked bright, and wanted to eat. Continued the injection. Portions of the tumor continued to slough off.

29*th.*—Both ligatures came away. Patient complained of "much soreness" about the genito-urinary organs: bowels moved freely during the day. Applied nitrate of silver, through a speculum, to the remains of the tumor; continued the injection per vaginam; substituted zinci sulphas, grs. x, aqua, ℥ j.

30*th.*—Was doing well. Prescribed tinct. nux vomica, gtt. xxx, ter. die, and cod-liver oil. A mixture of tinct. ferri. chloridi et quin. was substituted for the nux vomica after a few days, with the continuation of the cod-liver oil. Ordered generous diet; continued the injection; patient's skin being yellow, and the liver seeming not to perform its functions well, ordered four grains of pil. hydrg. every third night.

I omitted to state, that the catheter had been used to relieve the bladder, from the date of the operation.

From this time, the patient's improvement was rapid and satisfactory, regaining her strength and flesh in a few months. In December following, I examined Mrs. H.'s uterus, found it healthy, and occupying its normal position—no vestige of the tumor remaining, save a small elevation just within the cervix. To this I applied, freely, caustic potash, and advised an injection per vaginam, of tinct. of iodine (sat. sol.), gtt. xxx, aqua ℥ j.

Remarks.—The interesting features to be considered in this case are, the size of the tumor (supposed to weigh a pound or more, there being no means of accurately ascertaining its weight), the long and profuse hemorrhages, and the mistaken diagnosis.

In the London Medical Times and Gazette for July, 1857, is the report of two cases operated upon by Mr. J. B. Brown, of St. Mary's Hospital, both proving fatal.

Mr. Hutchinson, of the Metropolitan Free Hospital, says that large uterine tumors, according to Dr. Attee's experience, may be left to disintegrate with very good confidence (provided they have been well cut into and exposed); that such results will ensue, as some facts which have fallen under his observation lead him to give full credence to this *a priori* improbable statement.

The vitality of many uterine fibroids, continues he, appears to be very small, and comparatively little interference will often suffice to insure their death; whilst the manner in which large tumors, when they have become sloughy, will, as it were, melt away, is quite astonishing—as instanced in the case under consideration. Again, Mr. Hutchinson (than whom there is no better authority upon the subject) states, that surgical interference with interstitial fibrous tumors of the uterus is often attended with considerable risk, and ought not to be practised, except under circumstances of urgency, or when the position of the growth is peculiarly tempting.

The prime source of the hemorrhage is a feature, perhaps, in the pathology of polypus growths not hitherto decided. Lesfranc and others maintain, we believe, that the hemorrhage comes from the surface of the polypus; whilst Dr. Barnes, in some able papers in the Lancet, contends for the lining membrane of the uterus itself. Mr. Hodgson, who has paid much attention to the subject of arteries and hemorrhages, is of opinion that the veins of the polypus are compressed in its neck, and the hemorrhage is caused in this manner.

Mr. J. B. Brown, in his work on Surgical Diseases of Women, page 121, says it is difficult to explain the cause of the alarming floodings which attend the progress of these growths, and is inclined to think it depends much upon reflex nervous influence from the tumor, acting on the general surface of the uterus, and thereby causing the flux.

Dr. Tyler Smith is of opinion, that previous to the periodical and profuse discharges of blood, divers patches of the surface of a polypus become inflamed, and erosion, or the entire loss of the mucous covering, ensues. Next, that from the patches of erosion, blood is seen to escape freely, the whole surface of the

polypus bearing marks of frequent old inflammation, old thickening of parts, with patches of lymph, obolescent ulcerations, or old abrasions.

Dr. Smith's views were particularly illustrated in Mrs. H.'s case.

In reference to the mistake in this diagnosis, it would be a charitable conclusion to say, that the case never received its due consideration; doubtless, from the amount of pain which would have been inflicted by the necessary investigation.

Though the tumor was somewhat pear-shaped, it certainly bore a very faint resemblance to an inverted uterus; and such a decision must have been a forced conclusion, rather than the result of a thorough and deliberate examination.

From recent intelligence of this patient, I have every assurance of a perfect cure, there being no vestige of the tumor remaining, or any disposition to its recurrence.

CASE OF DYSMENORRHŒA.

Mrs. ——, aged about 28—married six years—has never borne any children; has suffered with dysmenorrhœa since the age of puberty, obtaining only partial relief at times from treatment, which has been very much varied by the different physicians by whom she had been attended.

Appearance at the time of my first visit: complexion pale, and rather sallow; stature rather below the medium; slight tendency to obesity. Her monthly periods have generally been attended with a good deal of suffering, and at times it amounted to complete agony. Leucorrhœa has been almost constant since puberty.

From her husband I learned that she had been treated in New Orleans for inflammation, and perhaps ulceration and hypertrophy of the uterine neck.

The first time I was called to treat her was when suffering with a violent attack of dysmenorrhœa, on the 16th of December, 1858. I found her in great agony. I immediately administered

half an ounce of syr. morph., and then gave her chloroform by inhalation, until the morphine could produce its effect. She was relieved for the time, and when the menstrual period had passed I determined to try a remedy I had seen recommended in the New Orleans Medical News and Hospital Gazette, viz., the tincture of antacrida.

The next period passed with much less suffering, as did also each succeeding period until she left home for a northern tour, in May, 1859. She informed me then that her health was very much improved. She continued its use but a short time longer, as it was inconvenient while travelling.

She returned home in September, in perfect health and in excellent spirits, and free from suffering at her monthly epochs. She informed me (to use her own language) that she was in better health than she had ever been since her recollection, and that the medicine I had given her had cured her. Another interesting feature of the case is, that there is a fair prospect of there being another member in the family in a short time.

This case is only offered to strengthen the evidence in favor of the medicine used. E.

ANTACRID TINCTURE.

℞.—Pulv. guiac., ℥ j;
Terebinth. canaden, ℥ j;
Hydrarg chlo. corrosivi, ℈ j;
Olei sassafras, *f* ℥ ij;
Alcohol, *f* ℥ viij.

Dissolve the corrosive sublimate in one-half the spirit, and then add to it the other ingredients. Of this, from ten to twenty drops may be taken, morning and evening, in wine or water.—*Ellis's Med. Form. of* 1854, *p.* 189.

Black Oxyd of Copper in Loose Teeth.—Dr. Hoppe strongly recommends, when the teeth have become loosened, rubbing some of the black oxyd of copper well into the gums every morning by means of the finger. This induces the contraction of the hyperæmic periosteal vessels, and the teeth again become firm. Various stimulating applications will produce the same effect, but with injury to the gums.—*Berlin Med. Zeit.*

ON THE DISEASES OF IREDELL COUNTY.

BY H. KELLY.

DURING the summer and Fall, epidemic dysentery, or flux, prevailed to a very considerable extent. During the months of June and July, we had measles in connection with it, which was very fatal. We had what I suppose to be an unusual complication with dysentery, viz., acute rheumatism, in many cases setting in, the third and fourth day after the attack, with great violence.

The treatment of dysentery consisted in the use of mercury, in the early stage of the disease: when liberally administered, it seemed to cut it short, and render the after treatment simple and easy, consisting of opiates, castor oil and ol. terebinth. When complicated with rheumatism, the treatment adopted was that usually pursued in that disease, except the use of the oil of lemons, which I have been in the habit of prescribing for the last six years, in every case of that disease that I have been called to see, and with more benefit than any other single remedy that I have been able to give. The dose is from ten to thirty drops every three or four hours, in any proper vehicle, and if nausea and vomiting should be brought on, lessen the dose. After the liver had been acted on by mercurials, then the bowels should be kept open by the administration (every day, if necessary,) of potassæ et sodæ tartras. I have given the oil at all periods of the disease, and with safety and relief. I have also used the bandage to the feet, ankles and knees, particularly when there was much swelling of those parts. When the feet and ankles are swollen, I apply the roller as high as the knee, having it renewed every twenty-four hours, and applied as tight as the patient can bear it. I have seen great benefit result from this in connection with the above treatment.

There was less bilious remittent and intermittent, or continued fevers, than for several previous years. Neither was there as much pneumonia or pleurisy. Their treatment, I believe, was that generally adopted in such diseases.

There was also less cerebro-spinal meningitis than in the year

previous: it yielded much more to treatment, and was not as fatal.

There was but little pseudo-membranous sore throat: that was likewise more amenable to treatment.

In some neighborhoods we had scarlatina, though presenting nothing of peculiar interest. The treatment, so far as I could learn — not having seen any case of it,— was the usual one.

During the autumn and winter we had a good deal of typhoid fever. There were two symptoms worthy of more note than is common in this fever, viz., violent pain from the occiput to the cervical vertebræ, continuing for several weeks, in many cases, and cerebral symptoms, of a grave character, lasting sometimes from two or three days to six weeks, producing a complete blank of the intellect for that time. There was nothing of interest in its treatment, except that ol. terebinth. was used with more freedom than is common, and in larger doses, and continued longer. I gave it, in a number of cases, quite early in the attack, and continued it throughout, and in not a single case so treated did hemorrhage occur: the cerebral, as well as all the other symptoms, were very much mitigated by this early exhibition of that remedy. I do not think we should wait for that condition of the tongue described by authors before giving this medicine, but commence its administration at a much earlier period than is usual.

In the majority of cases of this fever there were alarming hemorrhages, which required large doses of astringents and opiates, which should be frequently administered: after its arrest, the bowels generally became much constipated, and required large and oft-repeated doses of castor oil to keep them in a soluble condition. Convalescence took place slowly.

Notwithstanding the severity of the above-described fever, there was comparatively few deaths from its ravages. From repeated trials for the last ten years, I am pretty well satisfied that the use of disinfecting agents should be had recourse to in every case, or, at least, in every family where typhoid fever exists.

There is another prophylactic, of which I may say something in future.

CASE OF TRACHEOTOMY.

BY CHARLES H. HARRIS, M.D., OF MAGNOLIA, DUPLIN COUNTY, N.C.

A SHORT time since, I performed this operation, with success, upon a negro girl, some fifteen years of age, belonging to Mr. W. The case is of such interest, in the opinion of some medical friends, as to merit publication, and I yield to their solicitation in sending it, for that purpose, to our State Medical Journal.

Some six weeks before her master applied to me, the patient was seized with a violent fit of coughing while sitting at dinner. After this had partially subsided, she told her master that she had "*swallowed a bone the wrong way.*" The family physician was immediately sent for. Everything, short of a surgical operation, was done for her relief, but all of no avail. Her cough continued, her respiration became difficult, and her expectoration showing signs of blood and disorganization. She said that the last tasted of bone — a symptom she retained up to the day of the operation. There was a fixed pain at the inferior part of the larynx. On auscultation, I obtained the sound which usually attends croup. When I operated, there were present all the symptoms just enumerated, together with fever and great emaciation. Her voice was completely gone. her respiration barely sufficient to keep her alive, and her strength almost exhausted. She was rapidly declining, and, no doubt, would have died very soon, without surgical interference.

Dr. C. T. Murphy, residing not far off, and whose assistance I requested, coincided with me in the opinion that there was a foreign body in the larynx or trachea, and that an operation was necessary, as he had hitherto resorted, without success, to every other expedient to relieve the patient. We made every preparation necessary for the operation of Tracheotomy, deeming it unadvisable in this case to use anæsthetics. I then performed upon the girl (she having been placed upon a lounge, with her head thrown back and neck elongated) the usual operation for tracheotomy, beginning by making an incision from a point just below the cricoid cartilage to one near the upper end of the sternum. This incision passed through the skin, exposing the

superficial fascia; this I divided, and pushed aside with the handle of the scalpel. There was a little adipose tissue beneath it, which was treated in like manner, and then the sterno-hyoid muscles separated with the point of the scalpel, great care being taken to keep in the median line. The trachea was then opened, and the air rushed through the wound. I enlarged the opening with a probe-pointed bistoury.

We had occasion to stop several times during the performance of the operation, for the purpose of taking up arteries and reviving the patient. Towards the last she could not bear the recumbent posture, and much of the work was done while she was sitting erect. The difficulties which we had to encounter were numerous, and some of them formidable. Her extreme debility and difficult respiration, the swollen and turgid condition of the vessels as well as the muscles, the shortness of her neck, and the obscure developments of the larynx and trachea, were no trifling things to contend with. The pomum adami could scarcely be distinguished from any other part of the larynx, much less the cricoid cartilage and the tracheal rings. In the male, as is well known, the developments are more prominent than in the female, and more so in the old than in the young.

After enlarging the orifice, and allowing the patient time to recover a little from the fatigue of the operation, we made search for the foreign body. Dr. Murphy thought he could feel it with a probe bent for the purpose. I endeavored to catch it in curved forceps, but did not succeed. Several other instruments were tried with no better result. I then attached a piece of sponge to the end of a whalebone, introduced it into the wound, and carried it up into the larynx. After sweeping it around in the ventricles of the larynx, with a view of dislodging the foreign body, I then forced the sponge through the rima glottides into the mouth. The patient made signs, trying to tell us that the bone was no longer there, and afterwards, when able to speak, told me that I *pushed the bone into her mouth, and that she could not avoid swallowing it.*

The lounge was then moved near a window through which the sun was shining, and with a pair of curved forceps the wound

was dilated, exposing the interior of the trachea to a strong sunlight. We could see nothing unnatural. Our conclusion was that the foreign body was no longer in the larynx. We then closed the wound with adhesive straps. The girl remained with me a month, under my care and treatment. The wound healed very kindly; she improved steadily: and I returned her to her master entirely convalescent. I was afterwards informed by the master that her flesh and strength were rapidly returning, and her voice improving. Her general health is now good.

That there was a foreign body in the larynx is very evident, from the history and symptoms of the case. That it was removed during the operation is proved beyond doubt, by the sudden disappearance of her symptoms, the recovery of her flesh and general health, and by the improvement in her voice.

Fever, Typhoid.—M. Vrancken has recommended, in typhoid fever, ablutions with vinegar and water. M. Van Dromme (Rev. Med. Chir.) has employed this treatment largely, and with great success. He uses one part of vinegar to three of water, applied with a sponge over the whole body, night and morning. The diet is very low: pure or slightly acidulated water is permitted to be drunk *ad libitum*. Of twenty cases treated in this manner, one died. M. Vrancken also employed the acetate of ammonia internally; but this is regarded as useless by M. Van Dromme. Chomel has also recommended the vinegar ablutions.

M. Mazade (Rev. Med. Chir., Fev., p. 95, et Rev. du Midi) has employed the sulphate of quinine, in doses of 15½ grains daily, in seventy-one cases of typhoid fever, and comes to the following conclusions:—That this remedy is eminently useful when the fever assumes a remittent form; that it is also useful, but less so, when there are less regular remissions; that it is seldom useful, and often hurtful, in typhoid fever of continued type. M. Dupré (same journal) thinks that quinine is useful in continued fever, when of very adynamic type.

SELECTIONS.

Practice.

ON HYSTERICAL MUSCULAR HYPERÆSTHESIA.

BY M. BRIQUET.

Under the term Hyperæsthesia, or increased sensibility, may be comprised various conditions formerly designated as pains, neuroses, neuralgias, or nervous phenomena. Writers upon hysteria have passed very lightly over the condition, although it is so constant an accompaniment of the disease, that there is not a woman subject to hysteria that does not manifest it in one or more parts. In the present article M. Briquet confines himself to the consideration of *hyperæsthesia as it affects the muscles*, this being its form which has excited least attention. It is, however, of very common occurrence, and the ignorance that has prevailed concerning it has given rise to numerous and disastrous errors in diagnosis. It may excite surprise to learn that this hyperæsthesia not only constitutes a means of recognising the nature of hysteria, but a positive criterion in the decision in doubtful cases.

The muscular system must, indeed, be naturally expected to play a great part in hysteria, for the voluntary muscles are a common theatre for its manifestation, whether in the form of convulsive paroxysms, paralysis or debility, anæsthesia, or hyperæsthesia. The pains felt in the walls of the splanchnic cavities are of such common occurrence in hysteria that no observers have omitted to notice them, although failing to recognise their true seat. They have been usually designated "nervous pains," without farther explanation; and even the few authors who have recognized them as muscular have failed to appreciate the part that hyperæsthesia of muscles plays in hysteria. Yet from among 400 hysterical women examined on this point there were not more than 20 who did not exhibit such muscular pain. This hyperæsthesia is so easily recognized by the following signs, that it is surprising it has not been sooner appreciated:—

1. The pain is always located in places occupied by the fleshy portions of muscles. 2. As the superficial muscles are those usually affected, it is felt immediately beneath the skin. 3. Slight pressure or scratching with the end of the finger over the muscle induces or aggravates the pain. 4. The pain thus induced is very severe, causing cries or contortions on the part of the patient, or even a hysterical paroxysm. 5. Movement, and especially distension of the fibres of these muscles, produces or exasperates the pain, while rest abates or relieves it. 6. Feeble electrical currents, which scarcely produce a disagreeable sensation when traversing the length of a muscle in a normal condition, induce an amount of pain difficult to bear while traversing the hyperæsthetic muscle, and become quite intolerable when they are rendered a little stronger. 7. Under the influence of Faradisation, non-inflammatory muscular pains, such as rheumatoid, those of lead colic, and especially those arising from hyperæsthesia of muscles, are rapidly dispelled. This hyperæsthesia, which may be termed *hysterical myolgia*, does not affect all muscles alike, the superficial muscles, and especially those of the trunk, being most liable to it. In the trunk itself it has its places of preference, observed with so much constancy and regularity, as to serve for one of the signs of hysteria. The following are its various seats:—

1. *Cephalalgia.*—When the hyperæsthesia affects the muscles of the head, it constitutes what has been termed cephalalgia. So common is it in hysterical patients, that among 356 of these it prevailed habitually, or very frequently, in 300, affecting especially the frontal and temporal regions of the head, and in at least nine-tenths of the cases having its seat in the fleshy portions of the muscles. It is frequently pulsatile, and at other times lancinating, continuing even during rest in bed, which distinguishes it from the chlorotic headache, which is hardly felt except during motion.

2. *Epigastralgia.*—Hyperæsthesia of the muscles of the epigastric region plays a far more important part than has been suspected. It frequently occurs, having been noted in 817 out of 358 hysterical subjects. In 130 of these was no accompany-

ing derangement of the digestive functions, while in 187 such disturbance existed. In answer to the question, why there should be so constantly pain in parts which seem to have no relation to the moral emotions of the patient, it may be observed—(1.) Epigastralgia may be met with in the childhood of girls who are subject to ill-treatment, or who have a hereditary predisposition to hysteria, and in such it becomes associated with disorder of the digestive organs, and a disposition to migraine. (2.) In others it appears either at the period of the establishment of the menses, or later, during difficult menstruation. (3.) In a certain number of cases it arises amid the disturbances produced by chlorosis. (4.) In a few cases it arises during the evolution of the hysteria, without the aid of any especial cause. The above-named circumstances explain the origin of one-half the cases of epigastralgia, and the other half is due to the two following orders of causes. (5.) The hysterical paroxysms. These are usually preceded and accompanied by a feeling of compression, distension, or tearing of the epigastric region; and, for twenty-four hours after the fit is over, the patient often complains of epigastric pains. In proportion to the repetition of the paroxysms does the epigastralgia become established. (6.) Depressing moral emotions. These, when at all intense, produce painful constriction of the epigastrium. To sum up: this hyperæsthesia is due to the two orders of physiological causes, the direct influence of the nervous centres on the muscles, and the reaction of the disordered stomach on the same muscles.

Epigastralgia especially extends towards the left side, mounting up as high as the middle of the sternum, and rarely descending below the umbilicus. The pain is severe and continuous, and exasperated by emotion, the hysterical fit, progression or compression. The attitude is constrained, and sometimes the respiration is influenced, but the process of digestion does not aggravate the suffering. The ignorance that prevails as to the seat of epigastralgia has led to most serious errors in medical practice—these muscular pains having sometimes been thought to be localized in the solar plexus, and at others in the stomach; and, according to the doctrine of the day, have been treated as

neuroses, gastralgia or gastritis. Now, if we bear in mind that in somewhat less than half of the persons suffering from pains in the epigastrium the pain is only muscular, it is evident that that number has been treated for a gastralgia that had no existence. Moreover, in patients suffering simultaneously from epigastralgia and derangement of the digestive functions, the former may exist independently of the latter, and be easily relieved by means addressed specially to it; and yet the stomach has been needlessly tormented by treatment that should have been directed to the muscles. It is not meant to be said that the stomach itself may not be the seat of severe suffering, such as pyrosis, tearing pain, or distension, from indigestion; but such pains are intermittent and temporary, and should not be confounded with the continuous pains of the epigastric muscles. The writers on gastralgia never allude to this muscular pain; and as amidst the author's 358 cases of hysteria there were not above 10 in which gastralgia existed unaccompanied with epigastralgia, it is easy to see how often the stomach has been rendered responsible for ills that did not attach to it. Hysterical epigastralgia may continue as long as the hysteria itself, and persist to a very advanced age. When it prevails with intensity, the constant pain wears out the patient, and renders all movement insupportable. She becomes melancholic and emaciated, and presents all the appearance of premature old age. Fortunately, nothing is easier than the relief of the affection when appropriate means are resorted to.

3. *Rachialgia.*—Hyperæsthesia affecting the muscles of the back has been long recognized; and so common is it, indeed, in hysterical patients, that M. Briquet found it absent in 5 out of 311 cases. According to the details he gives, it is found 5½ times more frequently at the lower than at the upper part of the back, and is 4 times more common on the left than on the right side of the spinal column. It occupies in general a space corresponding to about 4 or 6 vertebræ. It usually appears subsequently to the epigastralgia, and may exist in various degrees, from a mere uneasiness, scarcely perceptible, unless pressure be made at the side of the spine, to the severest suffering, and consequent disturbance of important functions. When intense it

becomes very fixed, and is one of the symptoms of hysteria most difficult to remove, the pain recurring from the slightest cause. Its diagnosis is easy by means of pressure made on the muscles at the side of the spine, and by observing its usual connection with epigastralgia and other symptoms of hysteria. In spite of this, however, its presence has frequently given rise to the most serious errors, and a frequent one among these has been the mistaking it for an early sign of phthisis. What authors have described as tabes dorsalis, has often been but this rachialgia reacting on the viscera, and vitally affecting nutrition. But the most serious of all these errors is the mistaking it for disease of the spine, an error M. Briquet believes to be even more common than Sir B. Brodie has represented it to be.

4. *Pleuralgia.*—Pains at the side of the thorax are so common, that they have been noticed by almost all observers, but they have always been considered as neuralgic. In hysteria they are very common, having been found by M. Briquet in 223 of 300 patients in whom they were sought for. The seat is rather fixed, extending usually as a semicircle corresponding to the 5th, 6th, 7th, or even the 8th ribs, sometimes following their direction, and at others being still more oblique. It is six or seven times more frequent on the left side; and M. Briquet found it bi-lateral in only 19 of his cases. It usually comes on subsequently to the hyperæsthesiæ that have been mentioned, forming but an extension of them. Hysterical pleuralgia can only be confounded with pleurodynia, or the pain of pleurisy; and it suffices to be aware of the possibility of the former and the usual coëxistence of epigastralgia and rachialgia to prevent any error. It is more easily confounded with intercostal neuralgia, by which name many cases of hysterical pleuralgia have been erroneously indicated. The distinction is established between these as follows: (1.) Hysterical hyperæsthesia does not follow the direction of the nerves and their branches, and invariably is found at the 5th, 6th and 7th ribs, while the accompanying rachialgia and epigastralgia are situated above the points they would be observed at, if dependent upon neuralgia. (2.) The pain bears no resemblance to that of neuralgia, being excited at any point that

is compressed, without radiating along the course of the nervous tracts. (3.) Neuralgia setting out either from its point of origin or termination, soon extends to all the divisions of the affected nerve. Hysterical pleuralgia only shows itself after epigastralgia, and then rachialgia, have already long existed. Pleuralgia usually constitutes only a more or less considerable inconvenience, and however long it may persist is at last dissipated without having induced phlegmasia of the contents of the thorax.

5. *Cœlialgia.*—The hyperæsthesia is so termed when it has its seat in the walls of the abdomen. The pains produced have long been known to authors, but have usually been attributed to lesions of the sympathetic system. The author met with it in 196 of 400 cases of hysteria. In 42 it affected both sides, in 76 the left side alone, and in 34 the right side. The pain usually engages a considerable portion of the muscles affected, and is continuous while in the erect posture, being much increased by movement and pressure, and relieved by lying down. In many instances this affection has been mistaken for painful affections of the ovary, and it is such mistake that has given rise to the supposition that ovaritis is of more frequent occurrence on the left than on the right side. The superficial character of the pain ought to prevent this error being committed; and if we first relieve the mere muscular pain, we can then press over the ovary without inconvenience. The duration of this hyperæsthesia is very variable, the amount of exertion or repose taken by the patient, and the condition of the genital organs, much influencing this. The patients are often much distressed by the pain, fearing it to be indicative of uterine disease.

6. *Thoracalgia.*—Among the 400 cases only 27 complained of pain at the anterior part of the thorax. In 12 of these it occupied only the left, and in 2 the right side; while in 13 all the anterior portion of the chest was affected. This form of hyperæsthesia is usually one of the last that appears; and its rarity is probably due to the slight part which the pectoral muscles (wherein it is seated) take in the expression of depressing emotions. It is not very persistent, and rarely disturbs respiration, but gives cause to much anxiety from the patients' fearing that

they are the subjects of disease of the heart. Its independence of neuralgia may be judged of by the fact, that of 150 hysterical patients with rachialgia at the level of the first six dorsal vertebræ, only 27 complained of pain at the anterior part of the thorax.

7. *Melyalgia* (from *Melos*, a limb) is the term applied to the hyperæsthesia of the superficial, and perhaps the deep-seated muscles of the extremities. The existence of such pain in hysterical women has only been noticed by authors in a vague manner. Among the 400 cases only 58 examples occurred, both upper and lower extremities being affected in 13 of these, the upper alone in 21, and the lower alone in 24. It varies in degree from a mere disagreeable sensation to severe suffering, destroying all repose, and giving rise to fever. In spite of the most unfavorable appearances, however, it disappears sooner or later, either spontaneously or under the influence of treatment. It cannot often be confounded with neuralgia, seeing that in two-thirds of the cases the locality of the pains does not correspond with that of the origins of the nerves distributed to the muscles affected; while from the pains arising from affections of the nervous centres it may be distinguished by its aggravation through pressure. The distinction from rheumatic myosalgia is more difficult. It must be drawn from the nature of the pain and the concomitant circumstances. The pain in hyperæsthesia is usually excessively intense, the slightest contact causing very severe suffering, while rheumatic pain is much less increased by pressure, and this requires to be firmer. The hysterical pain is manifested in women who have already presented other symptoms of hysteria, and almost always coincides with other myosalgias. The emotions exert great influence upon it. Rheumatic pain usually appears in women who have already suffered from muscular rheumatism, and is less exasperated under the influence of emotion. The difficulty of the diagnosis would be increased if the patient were both hysterical and rheumatic; but then the inefficiency of the means which usually succeed in the hysterical myosalgia would suffice to determine the rheumatic character of the affection.

Two points will remain in respect to melyalgia. (1.) Does

it arise from inflammatory action, or from mere perversion of sensibility? The question is of importance, inasmuch as hysterical hyperæsthesia also affects the muscles of the splanchnic viscera, giving rise to accidents, the nature of which has been the subject of dispute, and which may be more easily determined by observations of muscles more superficially placed. There is none of the tension, pulsation or heat observed in inflammatory pain, and this hysterical pain may reach an incomparable intensity. It is sudden in its appearance and disappearance, obstinately tenacious in some cases, while in others it is instability itself; and it is under the direct and immediate influence of moral emotion. It is no wise influenced by antiphlogistic remedies, only yields capriciously to narcotics, and can usually only be relieved by special means that have no effect upon inflammation. (2.) Is this pain a neuralgia? This at first is a natural supposition, but it is unfounded. True neuralgias, comporting themselves exactly as do the pains of nervous trunks, are, in fact, very rare in hysteria; and besides, numerous other points distinguishing these from myosalgias, it is to be observed that pain is felt at the extremities of the nervous trunk, but not in the intermediate portion, while in hyperæsthesia of a muscle the pain is felt along the entire course of the part affected. In hysteria pain is only felt at the part compressed, while in neuralgia it is induced by pressure on certain points, and then irradiates along the course of the nerve affected. But the best test is the effect of treatment, for while the hysterical hyperæsthesia is easily removable, none can be more obstinate and tenacious than the neuralgic.

Hysterical myosalgia, in some of the various forms that have been noticed, is one of the most marked symptoms of hysteria, and as it is very rarely absent it may itself be set down as a characteristic of the disease. Whenever we observe very acute pain induced by simple pressure of the finger in a part of the body exhibiting no traces of inflammation, we may from this sign alone diagnose the presence of hysteria with certainty. Whenever we are in doubt in ill-determined cases, we have only to gently rub with the end of the finger the upper end of the recti

muscles to become convinced. Observation has proved that it is rare not to find hysterical hyperæsthesia, either at the epigastric region, high up on the back, or low down on the left side, and, in a very great majority of cases, in all three situations at once; and every woman presenting such combination may most positively be declared to be hysterical.

Treatment.—The means which have been employed for the local treatment of hysterical myosalgia may be grouped into four classes,—antiphlogistics, antispasmodics, anodynes and revulsives. From time immemorial, topical emollients, cataplasms, liniments, &c., have been had recourse to; but M. Briquet, after a persevering employment of this class of means, comes to the conclusion that they are of no use whatever, any benefit that has seemed to accrue really depending upon the temperature at which they are applied. The pain in this myosalgia is sometimes so fleeting that its disappearance in certain cases cannot be regarded as resulting from the means employed. Leeching, cupping or dry-cupping, are means that possess considerable efficacy, though far more as revulsives than as depletants. Thus, cupping is more useful than leeching, and the author has often had the glasses removed immediately after scarification, so as to secure the revulsive without the depletory effect. In fact, the loss of blood itself does not relieve the pain, while it induces debility, and increases the predominance of the nervous condition. A paroxysm of hysteria has not unfrequently closely followed an application of leeches. In former times, *antispasmodics* were regarded almost as specifics, and innumerable formulæ have been handed down with the recommendation of infallibility. M. Briquet has tried these various applications repeatedly, and has never observed any effect result unless irritation of the skin has been produced, or when the odorous emanations which have escaped from them have produced some modifications in the nervous system by becoming inhaled. *Narcotics* are not of much more use, a certain amount of amelioration being all that is procured. They require to be varied, and are but little to be relied on. Ice and cold water, which produce so much effect in dermatalgia, are somewhat less powerful in their agency when

the pain is deeper seated. They may be advantageously employed when the pain is superficially placed, as on the head, and when the irritability of the patients forbids the employment of stimuli, or when these have failed.

The truly heroic remedies in muscular hyperæsthesia are *stimulants*. These may be thus enumerated:—1. Very hot cataplasms. 2. Dry heat applied by cloths as hot as can be borne. 3. Chloroform and acetic ether, applied either together or separately. These act much more as irritants than as narcotics, and have the advantage of being applied over large surfaces for long periods. 4. Sinapisms. The effect of these is still more powerful, and applied for twelve or fifteen minutes in the same place, two or three times a day, they very frequently relieve the pain. 5. Frictions with tincture of iodine or croton oil, repeated twice a day, have the advantage over sinapisms that they can be applied over very large surfaces. 6. Blisters. These have more power than the preceding stimuli, and the pains of myosalgia will scarcely resist their influence. 7. But the means *par excellence* is the Faradisation of the skin, as performed by M. Duchenne. By his apparatus the electrical current is limited to the skin, its passage being accompanied by a series of sparks corresponding to the zinc pole. The pain these sparks induce is so severe as not to be bearable beyond a very few minutes. The hyperæsthesia is usually at once dissipated, and when this is the case, even firm pressure, or movements of various kinds, fail to induce any pain. Not infrequently a single application suffices, but sometimes the pains return at the end of an hour, but more commonly after five or six hours. If they have not returned by this last period, we may be certain that the cure is definitive. When they do return, the application must be repeated, they becoming feebler after each. If, however, after two or three *séances*, success has not attended them, the application must be renounced. The conditions for success of Faradisation are the recent date, the diffusion, superficial seat, and moderate insensity of the pain. Still, as these are cases in which, though the pain has been of old date, success has taken place, it may almost always be tried with some chance of benefit. As a gene-

ral rule, hysterical women bear Faradisation with far more courage than men; but some of them are so susceptible that its employment may even induce paroxysms of hysteria. In such cases chloroform must be previously inhaled, as it should be also in those cases in which, from the intensity and extent of the myosalgia, the patient may not be expected to be able to bear the Faradisation during the time necessary for its relief. The chloroform in no wise impedes the revulsive action of the Faradisation. Still Faradisation is a means we should not have recourse to until the others have failed, or when the pain is so slight that a current of small intensity will suffice for its dissipation. During the treatment of this affection, the repose of the hyperæsthetic muscles is of prime necessity, and in some cases has sufficed alone for a cure. In ignorance of the nature of the affection, and when the pains have been believed to be located in the splanchnic viscera, exercises of various kinds have been recommended. Absolute repose is essential, and a few days of such sometimes suffices to relieve pains that have resisted various energetic remedies.

It is to be understood that the local treatment of these mysoalgias is but the complement of the general treatment which is the basis of management of cases of hysteria.—*Union Médicale.*

—*Med. Times and Gazette.*

To Prevent Falling Hair.—In many cases of this troublesome complaint, the following mixture may be used with advantage:

℞—Castor oil.....1 pint.
Alcohol, 98·00.... ...1 "
Tinc. cantharides .....1 oz.
Oil bergamot.....½ "
" jessamine .....½ "
Tr. Benzoin comp. ....½ " M.

Apply to the scalp once every three or five days. Sometimes the tinct. cantharides may be replaced by double its amount of tinct. of horseradish.

CLUBBING OF THE FINGER-ENDS AS A SYMPTOM OF THORACIC DISEASE.

A VERY interesting case has been recently under care in Saint George's Hospital, of large subclavian aneurism, in which the circulation of one upper extremity had for long been greatly interfered with. We do not, however, now advert to its details any further than as they bear upon the right interpretation of clubbing of the fingers. The fingers of the affected arm had become clubbed in a very marked degree, while those of the other hand had retained their original shape. The man was not the subject of phthisis or of tubercular deposit in any form. It seems, therefore, clear that simple retardation of the circulation in the part is, if long continued, an efficient cause of this peculiar symptom.

Clubbing of the fingers, when not an individual peculiarity, but coming on during ill-health, has often been considered an indication of pulmonary phthisis. By what steps of causative influence it was produced no one knew. Some years ago, in the practice of the City Hospital for Diseases of the Chest, the writer took considerable pains to ascertain its value as an indication of phthisis, and to discover whether it was in any way connected with the tubercular dyscrasia. Several cases of chronic empyema discharging through the bronchial tubes, displayed it in a very marked degree, while in none of them was there any reason to suspect tubercular disease. The observation of some instances of cyanosis in which it was present, however, overthrow this hypothesis. The case of a lad under Dr. Barker's care, in Saint Thomas's Hospital, has in particular fixed itself on his memory in connection with this point. The boy had congenital malformation of the heart, and his face and extremities were constantly livid and blue. His fingers were remarkably clubbed, and so were all his toes, as is very commonly the case. He had never had any cough or expectoration. The occurrence of this symptom in the cyanotic removes it in a very positive manner from any necessary connection with the tubercular diasthesis, since, as is well known, blue blood is to a certain extent antago-

nistic to that condition. Thus, then, it seemed fair to conclude that a symptom occurring in phthisis, empyema and cyanosis, must depend upon some condition which these affections have in common, and not upon tubercle, which is present only in one, or on profuse expectoration, which occurs in only two. Retardation of the circulatory fluid and its insufficient aeration, seemed to be this one phenomenon common to them all, and in further support of this view was the fact that it is far from being universally met with in phthisis, and usually seems proportionate in degree to the evidences of pulmonary obstruction (purple lips, &c.).

It is needless, after the above enumeration, to point attention to the peculiar interest which Dr. Ogle's case possesses as furnishing a sort of crucial proof of the truth of the conclusion arrived at. The phenomenon in question being exhibited in it in one hand only, it was clear that it could not depend on a constitutional cause, but must have been induced by some influence peculiarly exerted on the affected member. There could be little difficulty, under the circumstances, in saying what this influence was. In future, therefore, we may safely conclude that clubbing of the ends of the fingers, with whatever form of disease it may be associated, is an evidence simply of impeded circulation and of deficient oxygenation of the blood. —*Med. Times and Gazette.*

Itch Ointment.—In the Atlanta Medical and Surgical Journal for September [1859], a "Retired Pharmacien" gives the following formula for itch ointment:—℞ : Brown soap, one oz.; salt, ½ oz.; sulphur, ½ oz.; alcohol, one drachm; vinegar, two drachms; chloride lime, ½ drachm. Mix. Apply thoroughly to the affected parts on going to bed at night. The mixture should be prepared for each case, as it does not keep well. Its efficacy has been well tested by the writer, as well as by others.

The above is probably not more effectual than the simple sulphur ointment; but, if properly scented, it would probably be less objectionable to the fastidious.

Surgery.

ON UNION BY THE FIRST INTENTION AND PURULENT ABSORPTION.

By Warren Stone, M. D., Professor of Surgery in the University of Louisiana.

The tendency of us all is to fall into a routine, and to adopt some mode of practice, and apply it to all cases and conditions; and hence the opposite systems of practice, often seen, not only in different countries, but in the same country and in the same city, and even in the same hospital. This is strikingly illustrated by the exclusive and opposite methods of dressing wounds adopted by the American and French surgeons. The American surgeons dress with the view of obtaining union by the first intention in all cases, whether it is probable it may be effected or not. The French surgeons fill their wounds with lint to prevent union, if there should be a disposition to it. The French, however, I believe, are not so exclusive in their mode of dressing as the Americans, but both are undoubtedly wrong. It is true that our mode of dressing is far better adapted to our country than to France, but it is evident that neither method is adapted exclusively to either country. The American surgeon dresses all cases alike, whether there is a prospect of immediate union or not. The wounded or divided parts are brought in close contact, regardless of the consequence of confining dead animal fluids upon surfaces not protected by the exudation of plastic lymph. The French surgeon, fearing purulent absorption, or the absorption of other impure animal matter, leaves the wound open, or fills it with charpie, to absorb any fluid that may accumulate, and to stimulate the surfaces. It is quite certain that, in the French hospitals, at least, adhesive inflammation does not take place so favorably as it does in ours, and I will not pretend to determine how far they are wrong in their exclusive method of dressing; but, favorable as our country is, I am sure that we are as much in error in our exclusive method as they are in theirs. We have not attached sufficient importance to the danger of confining by dressings the effused fluids in the depths of

wound that do not take on healthy, adhesive inflammation. Even if absorption does not take place and affect the system generally, the local effect is bad, causing unhealthy action, and in some cases favoring gangrene. This is shown by the unhealthy inflammation that often follows contused and lacerated wounds, where the blood is suffused in the tissues, and the air has sufficient access to it to favor decomposition.

Hunter, in his work on the blood, warns us against making an opening to discharge the blood that may be extravasated in the tissues after contused wounds, but without giving any other reason, except the fact that such opening is liable to be followed by an ill-conditioned inflammation. While blood is excluded from the air, it remains harmless, and plastic lymph will be exuded around it; but if the air has access to it, decomposition may change it to poison, which will change the character of the inflammation entirely, and reduce the blood to the capacity of the absorbent, and it may be absorbed, and produce more or less constitutional disturbance. I am sure this takes place, under certain circumstances, from blood extravasated into the large serous cavities, for I have seen cases of death, with all the symptoms of animal poisoning, shown by the experiments of Magendie, when the consequent inflammation was very little. This should furnish a plain indication in the management of wounds of this kind; and in the dressing of wounds generally, we should be cautious in confining the animal juices or effusions.

When union by the first intention cannot be expected, it is, at best, useless to bring the surface of wounds closely together, and even if no other serious consequence follows, the wound itself often takes on unhealthy inflammation, and does not granulate as well as if it had been left more open for the ready discharge of any effused fluid, and for the application of stimulants, if necessary.

It is a great object, in many cases, to effect union by the first intention, in large wounds such as are left for amputation, for the diminished irritation and the saving of the drain of suppuration may often be the means of saving the life of the patient. Something may be done before dressing to lessen the effusion that

always takes place from exposed surfaces, and to favor healthy reaction. The effusion of serum would cease, of course, if we waited long enough before dressing, but this is not generally done, and, indeed, in many cases it would require many hours' exposure. The application of ice answers the double purpose, that of drying the stump and affording healthy reaction and the adhesive process. This, I believe, is a perfectly safe application (unless rigors are produced by it), and when the capillary circulation is feeble, or when it is feared that too little reaction will take place, there is no other stimulant so wholesome, or that produces so natural and healthy a reaction.

In the French hospitals, purulent absorption is the great dread, and this has led to a more general use of the écraseur than would otherwise have been given it, for it is asserted (and I believe with truth) that wounds made with this instrument heal more favorably than those made with the knife, and are much less liable to what is termed in Paris, purulent absorption. I have reference only to the Paris hospitals, for union by the first intention cannot be contemplated when the écraseur is used. The favorable result, however, of wounds or operations by the instrument, goes to prove what I have asserted of the bad effects of the effused animal fluids in wounds, either directly on the surface of the wound, or by absorption. The écraseur does not make a rough and ragged wound, as is generally supposed, but the tissues are cut as with scissors, but in such a gradual manner as to give time to the closure of vessels of moderate sizes, and consequently there is no oozing or exuding from its surfaces, and it remains dry, and is followed by no ill-conditioned inflammation. Why is this, when incised wounds are disposed to behave so badly under the same circumstances, and in the same hospital? I can see no reason but the effusion of animal juices that takes place in incised wounds, which is prevented by the écraseur. The incised wound produces less violence, and if everything is favorable, union by the first intention will follow, which cannot be expected of a wound by the écraseur under the same favorable circumstances. The keen cutting instrument is far preferable in most, if not all cases, and so far as any absorption of im-

pure matter is dangerous, the effect can be entirely obviated by the application of ice in favorable cases, and, if necessary, the application of the alum or perchloride of iron, which arrests at once all oozing of the fluids, and produces a wholesome stimulating effect. These two applications are only recommended when union by the first intention is not expected, and when there is a general disposition to unhealthy inflammation.

I have been surprised at the readiness with which wounds heal when I have found it necessary to apply these potent styptics, and the uniform healthy character of the reaction after their use. The écraseur is better adapted to the removal of hæmorrhoids than anything else; but fifteen minutes to a half hour must be taken for each application, or there will be danger of hemorrhage. I have been in the habit of using scissors, and applying immediately a sponge of suitable size, saturated with a strong solution of alum iron, which arrests the bleeding with certainty, and the parts give very little trouble afterwards. In a pure state, there is not much difference in effect between perchloride and the alum iron, but the perchloride decomposes readily, gives down a sediment, and leaves an excess of acid, while the alum iron is always neutral. Whether the French surgeons mean by purulent absorption the actual absorption into the circulation of pus, or mean to express a pyæmic condition, I am unable to determine, though it is certain that they do not mean that pus, in its natural state, is absorbed, for it is decided that this cannot take place. They apply it to that pyæmic condition which is attended by great depression, and the formation of matter in various parts of the body, which they attribute to the absorption of impure matter from wounds, it may be disintegrated pus, or other animal fluids. This never takes place in healthy and vigorous subjects, in which healthy exudation of lymph follows wounds, and it is quite plain that its frequency in the Paris hospitals is due, in a great measure, to the want of fresh air and proper nourishment. I speak of the deprivation of fresh air from personal observation, and judge of the diet from the general appearance of the patients. In the London hospitals, surgical patients presenting the appearance of the generality

of those in the Paris hospitals, would be ordered beef, mutton, and port or wine, &c. This general poverty of the blood prevents adhesive inflammation, and leaves wounds in condition to absorb the unhealthy fluids that form in them, and the general system in a state to be most seriously affected by it.

By proper preparation, by means of tonics and proper diet, much may be done to prevent this unfavorable result. Several years ago, and for several years, operations were disposed to take on a kind of diffusive, unhealthy, cellular inflammation, and in all cases that admitted of delay, I was in the habit of preparing them, by the use of tonics and attention to the stomach and diet, and by these means I almost always avoided this unhealthy process. The late Professor Wedderburn, who was one of the visiting surgeons of the Charity Hospital, during the time referred to, made local applications for the same purposes. His usual application was a strong solution of the sulph. quinine, with a slight excess of acid, and he thought it almost a specific. I believe it was of some service, but the more potent applications before mentioned, I am confident are more sure, if the case should be imminent enough to warrant their use, and I hold that these applications should be warranted in all cases where union by the first intention cannot be expected, and when unhealthy inflammation is probable. These cases, however, are different from the cases of pyæmia in the Paris hospitals, and are probably due more to some epidemic poison and deterioration, than to poverty of the blood. Pyæmia has also at times been prevalent, as if some epidemic influence was operating. · Tonics, fresh air and proper nutriment, constitute the general treatment, and stimulants or potent astringents are proper local applications, and very effective, at least as much so as the écraseur.

—*N. O. Med. and Surg. Journal.*

Dr. Sims, of New York, recently extirpated the entire uterus, in a case of procidentia, using the *écraseur*, and relieving his patient.

Dr. Brainerd, of Chicago, amputated at the hip-joint for the third time, but the patient died in sixty hours.

SULPHURIC ETHER SUBSTITUTED FOR CHLOROFORM AT LYONS.

At Lyons, the second city in France, sulphuric ether has almost universally superseded chloroform, both in hospital and private practice; and as the result of a recent discussion at the Medical Society of that town, the following resolutions were passed unanimously: — "1. Sulphuric ether, employed as an anæsthetic, is less dangerous than chloroform, no accident, indeed, having followed its exclusive and abundant employment at Lyons during eight years. 2. Anæsthesia may be as abundantly and as completely induced by it as by chloroform. 3. If ether gives rise to inconveniences which are not produced to the same extent by chloroform, these are of little consequence as compared with the dangers inherent to the use of the latter. 4. Ether should, therefore, be preferred to chloroform." It was proposed that the fact of using chloroform should be stigmatized as imprudent; but the Society declined taking this step, contenting itself with declaring that ether fulfils the same indications as chloroform, without giving rise to the same dangers.

—*Med. Times and Gazette.*

ALCOHOL AND ALCOHOLIC PREPARATIONS IN SURGERY.

Alcohol coagulates albumen in whatever liquid it may be, and consequently in blood, the synovia of synovial membranes, of articulations, the serosity which bathes the meshes of cellular tissue, and that which moistens the surfaces of serous visceral cavities.

Alcohol applied to living tissues on the surface of a wound provokes no sort of accident. We are convinced of this from repeated experiments.

It coagulates albumen on the surface of wounds, forming it into a grayish-white-pellicule. It stops the hemorrhage of the small vessels. It hastens the secretion of plastic lymph on the surface of the wounds. By separating the lips of the wound soon after the application of the alcohol, we can watch the lymph

in the very act of gluing them together; that is to say, the same thing is taking place there which takes place on the surface of serous membranes.

From these facts and these principles it results that alcohol exercises a great influence on union by the first intention, that it prevents diffuse erysipelatous inflammation, purulent infusion of synovial membranes, and purulent infection.

Let us explain ourselves:

Alcohol favors union by the first intention, by stopping hemorrhage of the smaller vessels (since the blood is a great obstacle to a perfect coäptation of the parts), by immediately producing a coagulum on the surface of wounds, and by accelerating the plastic secretion.

It prevents diffuse suppuration by coagulating the albumen of the cellular tissue, which immediately becomes thick, and is rendered impermeable to the unhealthy liquids which bathe their surfaces.

Alcohol prevents the purulent effusions of synovial membranes by the immediate coagulation of the synovia, by causing the opposite surfaces to adhere, by rendering them impermeable to the liquids which moisten the surfaces of fresh wounds, and by encouraging an adhesive inflammation of the synovial membranes near the wound.

Alcohol prevents purulent infection, by coagulating the blood in the open and even gaping veins, on the surfaces of wounds, thus instantly plugging them, and favoring an adhesive inflammation.

We must, therefore, in dressing recent wounds, abandon the use of fatty bodies and poultices, and return to alcoholic preparations; in a word, we must return to the practice of the ancients.

—*Chicago Medical Examiner.*

Bleeding Piles.—One of the simplest and surest means of checking this kind of hemorrhage, is the application of perchloride of iron dissolved in glycerine, and applied on a piece of lint.

—*Med. Times and Gazette.*

IMMENSE ANAL PROLAPSUS.—NEW OPERATION—CURE.

THIS case was presented by Professor Hamilton, at the Penitentiary Clinic, December 7. An Irish lad, named Quin, aged twenty, pale and feeble, with great muscular relaxation, has been tormented with prolapsus for a long time.

The protruded mass was of an exceedingly florid color, and of the shape of a truncated cone. The ordinary length of the cone was full four inches; the base, resting upon the integument, was ten and a half inches in circumference. The truncated extremity was about an inch in diameter.

After careful and repeated consideration of all the circumstances of the case, it was determined to yield to the entreaties of the patient, and incur the hazards of the following operation:

The bowels having been thoroughly evacuated, and an injection of warm water containing a drachm of laudanum administered, he was directed to effect a medium protrusion. Placing him upon his knees, and throwing his trunk well across a chair, an assistant forced the nates well aside, so as to expose the tumor's base. With a long, large needle, made for the purpose, and loaded with a strong and rather large whip-cord, the two ends of which were kept even, one being colored with ink, the base was now transfixed perpendicularly, so as to carry the needle, and of course the two cords, through a ring, attached to a handle by a shaft, and carried up into the cavity of the tumor. The ring was now pulled out, bringing with it, of course, the white and black cord. These were now cut in two, giving four ends hanging from the interior, and an equal number from the exterior. In the same manner, a transfixion was now made, in a horizontal direction, doubling the number of ligatures hanging from the interior, and exterior, respectively. By a little reflection, it will be seen, that by tying two vicinal strings of the same color, which hang from the inside, then drawing on the extremities of the same which hang from the outside, the entire ring of protruded issue, constituting the base, may be ligated and strangulated in four parts, which will be equal, if the transfixions are accurate. Furthermore, it will be observed that the traction

necessary to the induction of strangulation acts equally toward and from the centre of the anus, and that accordingly it is not occluded by the operation. After its completion, the index finger could readily be passed within the ligatures.

Twenty-eight hours after the operation, the pulse had reached 128 per minute; at the end of forty-five hours it numbered 114, and vomiting, which had been a prominent and distressing symptom, had subsided.

At the end of four days the pulse was 96 per minute; patient was resting fairly; had but little pain, and was relishing very fair supplies of food. At this time the protruded mass was distinctly shaken, synchronously with the pulse. The skin about the anus, perineum and nates seemed perfectly healthy.

From this time to the end of the eighth day, there was no change worthy of note. Sphacelation of the summit, with separation of the slough, leaving about one-half of the length of the tumor, had now occurred, leaving a surface which exhibited a degree of sensibility and vascularity, not admitting of the reasonable expectation of further sloughing from the first ligation. The ligatures were accordingly withdrawn. Ulceration had occurred about them to such an extent, that the orifices from which they were withdrawn would readily admit of the introduction of a finger. The intervening masses of tissue, embraced within the ligatures, were reduced to the size of a finger, and were very dense. A curved probe, armed with a ligature, was now passed readily around these masses, and they were subjected to a second ligation.

From the ninth to the eleventh day, inclusively, the patient was much more comfortable than at any previous time. Previous to this time, the catheter was in daily requisition, but urination was now readily effected. The pulse ranged at about 80; patient rested well, with almost perfect exemption from pain; on each of the two latter days there were fecal evacutions. Discharges of flatus were present from the first, and throughout the case.

On the thirteenth day, three of the ligatures last applied had separated. Daily fecal evacuations had occurred for several

days; the remaining ligature, and the remnant of the protruding mass, were about ready to come away, and the general condition of the patient was, in every way, such as not to admit of a remaining doubt as to the success of the operation.

—*Ohio Med. and Surg. Journal.*

EMPLOYMENT OF IODIDE OF POTASSIUM IN THE TREATMENT OF ANEURISMS.

The Gazette des Hôpitaux has lately reported a clinical lecture of Professor Bouillaud upon aneurisms, and upon the results of the treatment of aneurismal tumors by the iodide of potassium. One of the patients was a man suffering under an aneurism of the brachio-cephalic trunk and aorta, and the other was a woman with an aneurism of the carotid artery. In the latter case, the iodide of potassium was administered for some days in the dose of a gramme, and afterwards in doses of two grammes for two months. At the end of this period, the tumor, which was at first as large as a pigeon's egg, had diminished so much that it might be considered to have disappeared completely. In the case of the man, the tumor, which was of considerable size, underwent displacement contemporaneously with a very well-marked diminution in volume, under the same treatment as that adopted for the woman. At the time of the report, however, the man was still under the treatment by iodide of potassium, and therefore no positive conclusions could be drawn. M. Bouillaud has treated other cases with iodide of potassium. In the case of a man with a large aneurismal tumor at the point of origin of the carotid and subclavian arteries, he found that the swelling was considerably diminished in a few weeks by the use of the iodide. In another patient, treated in the same manner for an aneurism of the carotid artery, he observed at the end of a few weeks that the tumor had almost entirely disappeared. These cases are considered to be sufficiently satisfactory to encourage practitioners in making further trials of the iodide of potassium in aneurismal tumors.

—*American Journal of Med. Sciences.*

TREATMENT OF GONORRHŒA AND GLEET WITHOUT COPAIBA.

Mr. Weeden Cooke read a paper on this subject before the Harveian Society, January 19th, 1860. After referring to Sir Astley Cooper's method of treating gonorrhœa, the author stated his belief that he was only giving utterance to a largely-acknowledged experience when he ventured to affirm that copaiba, in the treatment of gonorrhœa, was not only unnecessary, but that it was in a great many instances injurious, and that in all it was offensive to the last degree. During the last fifteen years at least six thousand cases of gonorrhœa had come under the author's care at the Royal Free Hospital, and he had availed himself of these large opportunities to test all the methods of treatment which had been suggested, in order to arrive at the safest, quickest, most efficacious, and least disagreeable mode of curing this disease. As introductory to the subject of treatment, Mr. Cooke discussed the subject of the cause of chordee, the reason for the scalding of the urine, the distinction between true and spurious gonorrhœa, and the time when infection may or may not be apprehended. The conclusions he arrived at were—

1. That chordee, in ninety-nine cases out of a hundred, was due to spasm, and not to effusion of lymph; and that cubebs, acting as an antispasmodic, formed the most efficacious remedy for this symptom.

2. That the scalding was the result of the acid urine passing over the highly inflamed mucous surface of the urethra; and that this was to be remedied by the administration of the alkaline carbonates for the purpose of neutralizing the acidity of the urine, thus removing the principal cause of the continuance of the inflammation.

3. That, in all disputed cases, the true gonorrhœa may be known from the spurious by the presence of redness, heat, pain and swelling, together with a purulent discharge, more or less green and offensive; whilst a discharge produced by connection with a person who has leucorrhœa, or has recently been confined, will not be accompanied with these inflammatory signs, and the

discharge will be milky in consistence and color, differing much from the thick purulent discharge of the veritable gonorrhœa.

4. The time when infection may or may not be apprehended was discussed, in its incubative stage and at its close. A case was given, showing that the disease may be caught from one person, and not communicated to another two nights after, because the purulent discharge had not commenced at the time of the second intercourse. Respecting infection at the close of the disease, the author had been enabled, from experience, to establish as a law for his own guidance, that gleet—*i. e.*, a mucous discharge from the urethra, consequent on gonorrhœa—does not set up gonorrhœa in another person; but that, whilst any pus is to be found in the discharge, there is probability of infection.

Passing on to the subject of treatment, Mr. Cooke said that, upon inquiry at the London Custom House, he found that 118,396 pounds of copaiba were admitted into the port of London only during the first ten months of the year 1859. If this be administered at the rate of half a drachm three times a day, and supposing each patient takes it for three weeks, we have here copaiba enough to treat 473,584, or close upon half a million persons, and that in ten months only of the year. Considering how often it fails to cure the disease; how frequently it is rejected by, or at any rate disorders, the stomach; how tell-tale and disagreeable is its odor from the mouth and skin; how occasionally it produces a papular eruption all over the body; and that, in many instances, swelled testicle and stricture may be traced to its irritating influence, while gonorrhœa, rheumatism and opthalmia have been attributed to its administration; "considering all these objections," the author remarked, "is it not extraordinary that this disgusting medicine continues so long to hold its ground?" The abortive treatment by strong injections of nitrate of silver had proved a failure, because in some instances inflammation of the bladder had resulted. The treatment by diluents was slow in its action, and not readily employed by persons engaged in active business. That by diuretics was scarcely more successful; whilst the administration of saline aperients was generally attended with an aggravation of the ardor

urinæ as well as chordee. The treatment which had been most successful in the author's experience was the chemical treatment by the alkaline carbonates, given with a view of neutralizing the acid in the urine. Thus one great source of irritation was removed from the inflamed urethra, and the subsidence of the inflammation, which nature would effect, was allowed to take place. As auxiliaries, especially when there is œdema of the prepuce, lead lotions, and elevation of the penis against the abdomen, were commended. The inflammation having subsided, and a muco-purulent discharge being left, the author had found, after having given trial to all the injections which have been at any time in vogue, that the chloride of zinc, introduced into this branch of practice by Mr. Lloyd, of St. Bartholomew's, was the most efficacious of any in curing the disease, and that with less discomfort and in a much shorter time than by any other means. Since employing this treatment he had had little, if any, orchitis amongst his patients. The strength of the injection he most commonly employed was two grains to the ounce, but in some instances one grain to the ounce was sufficient. Whilst advocating this treatment in persons of healthy constitution, it was necessary to completely change it in others. In the strumous, in the dyspeptic, in those of dissipated habits, and where the diseased person is an old offender, the alkaline carbonates are not called for, because either the urine is not acid, or the inflammation does not run high. In such cases the tincture of iron, or sulphuric acid and bark, or gentian, or calumba, may be advantageously employed from the commencement; and the chloride of zinc injection, in these cases, is also of the utmost value in rapidly overcoming the disease.

Respecting diet, the author considered that after the subsidence of the inflammatory symptoms scarcely any restriction need be enforced, and that beer or wine in moderate quantities may be advantageously used by those who are accustomed to these beverages. He had found long-established cases of gleet yield readily to the chloride of zinc injection, accompanied with tonic treatment and generous living.

In conclusion, he would rejoice if the treatment he had found

so serviceable should be followed out by others, and thus assist in banishing altogether from surgical practice the use of so nauseating a drug as copaiba. —*Am. Journal of Med. Sciences.*

LIGATION OF BOTH COMMON CAROTID ARTERIES, AT AN INTERVAL OF SEVENTEEN DAYS, FOR EPILEPSY.

Dr. Gustav C. E. Weber reports (Cleveland Med. Gaz.) a case of this. The subject of it was a German, twenty years of age, who had been the victim to epilepsy for five years. When he entered the Cleveland City Infirmary his attacks came on daily; but his constitution seemed to have suffered very little, and his intellectual powers were not diminished. The patient not having been benefited by treatment, Dr. Weber determined to ligate the left common carotid artery. This was performed on the 2d December, 1857. The artery was secured below the omohyoid muscle. The ligature was tightened gradually and slowly, and produced no untoward cerebral disturbance. Twelve days after the operation the ligature came away, and a few days afterwards the wound closed entirely.

For seven days the patient was free from fits, felt completely relieved from his former distressing symptoms. The headache was gone, the paresis of the arm disappeared, and he could speak as distinctly as any one.

On the eighth day after ligation of the artery the patient had three fits, and subsequently they became frequent, though not so severe as before.

Thinking the first operation had benefited the patient, Dr. W. decided to take up the other carotid — which was done on the seventeenth day after the first.

"For five weeks after this second operation," says Dr. W., "the patient was entirely free from fits. They then returned for a time daily, and very lightly indeed, consisting, in fact, simply of short moments of unconsciousness, and a few convulsive movements of the muscles. Gradually they increased in severity, but diminished in number. For more than half a year he

had an attack only once a week, or every two weeks. Since then, again, they became slowly more frequent, and now, nearly two years after the first surgical interference, he has an attack every two or three days. They are, however, by far not so violent as they were before the arteries were taken up."

The patient enjoys good health, "but his intellect seems to become a little impaired;" and Dr. W. says he entertains no doubt but that in time he will become idiotic.

[The result of this case seems to us to afford little encouragement for an imitation of this practice].

COUNTER-EXTENSION WITH ADHESIVE PLASTER.

Rufus, a servant of Mr. John Davis, was placed under our care on June 16th, and upon a special examination with my partner, Dr. A. N. Talley, a comminuted fracture of the lower third of the shaft of the femur was discovered. With the assistance of two of our students, extension was effected, and after coäptation of the fragments, the roller bandage was applied to the entire limb. Three splints were next fitted to the thigh, and Welch's long extension apparatus was then adjusted by a gaiter, to secure the ankle to the footboard beneath, and a silk handkerchief, wrapped around a roll of cotton as a perineal bandage, tied firmly to the upper end of the long splint above the crest of the ilium. By means of a screw connected with the apparatus, extension was now made, until the measurement from the anterior spinal portion of the ilium to the internal malleolus was found to correspond to that of the sound limb.

The muscular development of the thigh afforded great resistance, yet the extension was steadily maintained for a week, when there was such excoriation from the perineal bandage as to induce a discontinuance of this mode of counter-extension for the application of adhesive plaster, on the principle, but not precisely in the place, suggested by Dr. Gilbert, of Philadelphia.

A strip of strong linen plaster, two inches wide, and two feet in length, was doubled upon itself at an angle, so as to make a

loop, and yet both of the extremities having their adhering surfaces in the same direction. One of the free ends was pasted closely over the spinous process of the ilium, and extended down towards the inguinal region; while the other end was carried down and pasted over the trochanter, so that both pieces had firm attachments, by the *closeness* of the *integument* to *these bony prominences*, and thus presented resistance to traction upon the loop above. Transverse strips were placed from one piece to the other to secure their adhesion. A piece of homespun was now passed through this loop, and made fast to the upper end of the long splint, thus effecting counter-extension. When the apparatus was tightened by the screw, it was found that all the indications were met satisfactorily, while the excoriated hip and the perineum were left entirely free for such applications as the ulceration required. The strips adhered closely for three weeks without interference, and have proven an efficient means of counter-extension.

The only particular in which my proceeding is entitled to any superiority over the process of Dr. Gilbert is, that it makes the attachment of the strips extend over the firm, unyielding points of bone, and thus serve, perhaps, to give a more fixed resistance to the force of extension. But while this is original with me, I prefer no claim which shall lessen the appreciation of the eminent service which has been rendered to the profession by the elaborate articles above referred to, and I trust that all may profit by their perusal, and cease to punish the subject of fracture with the excoriations of a perineal bandage.—*Am. Journal of Med. Sciences.*

Ligature of the Femoral Artery for the Cure of Elephantiasis of the Leg and Foot.—Doctor T. L. Ogier reports (Charleston Med. Jour.) a case of elephantiasis of the leg and foot, of enormous size, which had existed for five years, in a negro man, twenty-six years of age, in which he applied a ligature to the femoral. At the time of the report, three months since the operation, it is stated that "the leg and foot have subsided to very nearly the natural size."

Midwifery.

CHLOROFORM IN MIDWIFERY.

(Read before the College of Physicians and Surgeons of Louisville, as introductory to a discussion of the subject.)

BY R. C. HEWETT, M. D.

The first effects of the inhalation of chloroform are various degrees of paralysis of the nerves of sensation. In a higher degree, its influence reaches the medulla obligata, and the respiratory system becomes affected. If, after these effects appear, the inhalation of chloroform be still continued, the organic system of nerves becomes implicated, and death may result from paralysis of the heart. Thus we have in chloroform the means of producing all degrees of paralysis, from a slight insensibility to absolute death. The first impression of this agent is upon the ultimate ramifications of the sentient respiratory nerves, and in its more remote effects it enters into and modifies the condition of the blood. In its use, the question to be solved is, in what degree may we safely impress the nervous system, and how far may the blood be modified with impunity? A satisfactory solution of this question can be worked out only by a long series of such observations as are now being made in the use of chloroform, in all parts of the civilized world.

In my observation where *pure chloroform* is used, the great majority of persons become quietly insensible, without the stage of excitement which sometimes precedes the sedative effects. I am satisfied, that in most cases, if not all, in which such excitement appears, impure chloroform is used, or the patient, at the time of beginning the inhalations, is in a state of alarm and excitement, or is surrounded by disturbing influences of some kind.

A lady of this city requested me to give her chloroform, in order to have some teeth extracted, and made an appointment for a certain hour of the next day. When I met her at the time designated, I found, from her appearance and manner, she was much excited, and her pulse was greatly accelerated. In this condition she commenced inhaling chloroform, and after a few

inspirations became violently excited, threw herself out of the dentist's chair, and all the influence of her husband could not induce her to continue the chloroform, or even to remain in the room. I was satisfied she had become excited by anticipating the dental operation so long before the appointed hour arrived, and I told the dentist and her husband that we would take her by surprise, on some day to be designated by them, and give her chloroform before she would have time to become excited. This expedient succeeded completely. She readily came under the sedative influence of chloroform without the least evidence of excitement. Hence, before commencing inhalation, the patient should be made as calm as possible by assurances of perfect safety, and all noises and other disturbing causes should be removed.

In administering chloroform, I usually employ a hollow cone made of a sheet of paper, with a base large enough to cover the nose and mouth, and having the apex torn off so as to admit the air freely. Into this cone a small cambric handkerchief is placed, upon which the chloroform is poured. The advantages of this arrangement are, that the chloroform does not come in contact with the lips and nose, and thus produce a disagreeable smarting, and when inhalation is suspended evaporation is checked, and the chloroform is retained by pinching up the two ends of the cone. The patient's stomach should not be distended with food when chloroform is used, nor is a too protracted fast desirable, as depression of the vital energies from any cause makes the unfavorable influences of chloroform more liable to occur. During the first inhalations, the chloroform should not be applied close over the mouth and nose, and the patient should be carefully watched to see if any idiosyncrasy exists, forbidding the continuance of the inhalations. That such idiosyncrasies do exist in some cases, perfectly independent of any organic disease, I have no doubt.

The degree of anæsthesia most favorable for the complete safety of the patient, is that in which sensation is in a great measure suspended while consciousness remains. Patients frequently say, they have enough chloroform for the present, and

evidently suffer very little pain, although conscious of all that is going on. Carried to this extent, anæsthesia does not seem to impair the motor forces of the system in any appreciable degree. If the sedative influence be urged on to a greater extent, consciousness and voluntary motion are lost, slight stertor appears in the respiration, the pulse becomes slower and the eye is turned up. In a still higher degree, all these effects are deepened, and the conjunctiva may be touched without any evidence of sensation. When this degree of anæsthesia is reached, it has been errried as far as the boldest may dare to venture, for in such symptoms we have evidence that the organic system of nerves is becoming implicated, and complete anæsthesia of that is death.

One fact in connection with the use of chloroform should never be forgotten, and that is, that its effects continue to increase in intensity for some seconds after the inhalation of it has ceased; hence in giving it we should, if possible, stop the inhalation short of the full effect we wish to produce.

One of the alarming effects of chloroform which, however, I have never witnessed, but which is mentioned by writers on the subject, with sufficient frequency to show that it is not a very rare occurrence, is a sudden death-like pallor, with interruption of the circulation and respiration, coming on without any premonition, and, in some cases, after a very few inhalations of the anæsthetic. I look upon this accident as syncope produced by chloroform, and depending, most probably, upon some idiosyncracy of the person in whom it occurs. It is not very evident how the effect is produced. We know, however, that some persons faint from disagreeable intelligence suddenly communicated, and others from an unpleasant sight or odor. May not the disagreeable sensations incident to the first inhalations of chloroform disturb the function of the cardiac nerves, producing syncope in the same manner as it is often produced in certain idiosyncrasies by an offensive odor, &c.?

Whatever may be the true explanation of this accident, it is, in my opinion, syncope, and the heart is at fault. It should be treated by quickly depressing the head, elevating the inferior

parts of the body, and by dashing water in the face, or by passing a current of electricity through the heart, &c.

In other cases, asphyxia occurs as the result of an undue proportion of the anæsthetic influence being displayed upon the respiratory nerves. Artificial respiration is the proper remedy for this condition.

A few weeks since, Dr. Bayless and I were called to a case of dislocated shoulder joint. We found the patient suffering intense pain. He told us the same kind of accident had happened to him frequently before, and that he had generally been put under the influence of chloroform to have the luxation reduced. We placed him upon the floor, and commenced administering chloroform. After four or five inspirations, the respiration suddenly stopped, and remained so for fully twenty seconds. By shaking him briskly, respiration was restored. During this pause in his breathing, his pulse remained perfectly normal. As there was no insensibility as yet produced, we determined to try the chloroform again; but, as before, the respiration stopped, and we were glad enough to commence manipulating the shoulder, so that the pain might arouse the respiratory functions. We advised him never to use chloroform again.

In midwifery, it is difficult to determine, from all that has been published on the subject, what is the exact influence exerted by chloroform upon the parturient process. Some claim for it, that it produces insensibility only, while others maintain that it diminishes the motor action of the uterus. Some object to the use of anæsthesia as being liable to retard labor, and as favoring the occurrence of uterine hemorrhage. Others deny that it has any such tendency, but strangely enough teach that chloroform is a most valuable agent in removing the resistance of the uterus in operations involving the introduction of the hand into its cavity. One writer condemns the use of anæsthesia in ordinary natural labor, but thinks it may be used with great advantage in all obstetric operations, and especially in turning. Now, if chloroform is proper in such operations as turning, in which, to be of any service, the impression produced must be vastly more profound

than in natural labor, why is it more hazardous in the latter case than in the former?

My own impressions as to the use of chloroform in parturition are, that a considerable degree of insensibility may be produced short of suspending consciousness, and yet the motor forces may not be at all affected. If the impression be carried so far that consciousness is lost, motor force is impaired, and uterine power is diminished, but, of course, not to so great an extent as that of the voluntary muscles. We may admit that the expulsive power of the uterus is lessened by chloroform, and yet not concede that, as a general rule, it retards the progress of labor, for it also relaxes the vagina and os externum, so that what is lost in motor force is compensated for by diminished resistance. There are some cases, however—and I have certainly seen more than one—in which the use of chloroform had to be suspended on account of its evidently retarding and enfeebling the uterine contractions so as to delay the termination of labor.

A very limited experience with chloroform in midwifery must soon satisfy any one that it cannot be used so as to produce the same degree of anæsthesia in all cases. Every accoucheur must have often noticed the wonderful difference there is in the force of the uterine contractions in different females. Some years ago a case occurred in my practice, in which there was mal-position of the fœtus, and it was deemed proper to attempt turning. There was most powerful uterine action, and the uterus was so firmly contracted about the fœtus that it was utterly impossible to introduce the hand. With a view of diminishing the force of the uterine contractions chloroform was very freely given, and most profound anæsthesia was produced and continued for a long time, but not the slightest impression was made, either upon the force or frequency of the contractions. In cases having such excess of uterine force, chloroform may be freely used. But other cases are seen, in which the propelling power of the uterus is so feeble that the expulsion of the fœtus seems to be accomplished, in a great measure, by the voluntary efforts. In labors of this character, we can safely produce anæsthesia in a very limited degree only.

An objection has been made to the use of chloroform in labor, on the ground that pain in parturition is but a fulfillment of the primeval curse upon our race, and that we have no right to undertake to suppress it. If this be correct reasoning, we are equally debarred from doing anything to mitigate the suffering of childbed. But surely, in spite of all we can do in allaying pain, there is suffering enough during gestation, labor and convalescence, to fully answer all the conditions of the original penal decree. Besides, the great amount of suffering attendant upon labor in civilized life is an abnormal condition, incident to the cultivation and refinement of the nervous system, and as such we very properly combat it as disease.

In my opinion, all parturient females have the right to demand of us the employment of all means that can be safely used, either to facilitate labor, or palliate the suffering incident to it. Hence, if there be no counter-indications, we should permit the use of chloroform in all cases in which it is demanded. But judgment must always preside over its administration, determining when it should be withheld, and how far its effects should be carried. And we should never forget that its action may produce death, and that there are idiosyncracies that greatly increase the tendency to such a result.

In ordinary labor, anæsthesia should be produced in that degree only in which sensation is in a great measure obliterated, and yet consciousness to a considerable extent remains. As a general rule, I do not begin administering chloroform until the second stage of labor has commenced. The objection to its use in the first stage is, that the long-continued employment of it may impair the uterine force, and thus retard the completion of labor, and perhaps favor the occurrence of hemorrhage.

In using chloroform, the patient is permitted to inhale it, cautiously at first, until she becomes quiet, or ceases to make complaint of pain. Its use is then intermitted, until her movements show that another contraction of the uterus is coming on, when the inhalations are resumed, and continued to the same extent as before, and so on until labor is terminated. The *quantity* of chloroform employed in a given case is not to be regarded so

much as the effects produced. Three or four inhalations of it will affect some persons, as much as others will be impressed after continuing its use two or three minutes.

There are cases, however, in which there is an unusual degree of excitability, and in which the suffering of the patient is greatly out of proportion to the progress made in the labor. In such cases I have given chloroform before the os uteri was dilated, and in some instances with marked benefit, not only in allaying suffering, but evidently in expediting the parturient process.

In breech presentations I would not give chloroform, unless I felt assured that the uterus possessed such ample expulsive energy that its power would not be likely to be diminished so much that there would not be force enough to assist in expelling the fœtal head.

I have not observed any unfavorable effect from chloroform in parturition, except that in a few cases it evidently impaired the force and frequency of the uterine contractions, and thus retarded the progress of labor. Still, when this accident occurs, the uterine energy is usually soon restored by suspending the use of the anæsthetic. In all other respects I have found chloroform to produce the most favorable results. I have always noticed, on the day succeeding delivery, that those women who use chloroform are less prostrated, more cheerful, and suffer less from muscular soreness, than those who get through their labors without the aid of anæsthesia.

Many unfavorable occurrences are, I doubt not, ascribed to chloroform, with which, in truth, it is in no way chargeable. A case of most profuse post partum hemorrhage occurred in my practice, after the free administration of chloroform, and I at once mentally charged the dangerous condition of my patient upon the anæsthetic; but I subsequently learned that in her several previous labors, in which chloroform was not used, an equally copious hemorrhage ensued.

Out of the many thousands of parturient females who have used chloroform, some few may perhaps have died from its effects; but is it not very probable that out of the same number

of women in labor, as many or more would have died of *shock*, if anæsthesia had not been produced?

It has been claimed for chloroform, that its use in labor has been very rarely followed by puerperal peritonitis. I would be glad to hear the experience of the College on this subject.

CATHETERISM IN THE FEMALE.

BY ROBERT BATTEY, M. D., OF ROME, GEORGIA.

THERE is, perhaps, no minor operation in the whole catalogue in which expertness in the operator is more highly appreciated by the patient, and a blundering awkwardness so seldom forgiven, as catheterism in the female. To perform this little office acceptably, two things are necessary—1st, a full acquaintance with the anatomy of the parts to be operated upon; 2d, an educated sense of touch, which will enable the practitioner to determine definitely the structure with which it comes in contact, without resorting to that most humiliating expedient—aiding the hand by the use of the eye. A convenient and successful mode of accomplishing the object is the following:

The operator having washed his hands, whether clean or otherwise, by way of assurance to the patient on this point, as also to soften them, and render the sense of touch more acute, takes his position in a chair to the left of the patient, who should be upon her back, conveniently near to the edge of the bed. He now anoints the catheter with oil or lard, using the index finger of the left hand, holding the instrument in his right, between the thumb and middle finger, with the end of the index closing the mouth of the catheter; raising the cover a little, he passes both hands under it, the left to the vulva, beneath the flexed thigh of the patient, the right to the symphysis pubis. Introducing the left index into the vagina, with its palp upon the urethra, he draws the point of the finger along the under surface of the canal, until it drops off at the end—the meatus urinarius, which he thus locates definitely in his mind: now, passing the finger back into the vagina, and resting it upon the whole length

of the urethra, he places the point of the catheter against the finger as a guide, slides it down gently into the meatus, and through the urethra into the bladder, fully sensible, the while, of the correctness of the manœuvre, by the impression made upon the finger in the vagina. If the first attempt should be unsuccessful he withdraws the instrument and tries again, using more care to keep the point in the median line.

In this and other manifestations in and about the vagina, the sense of touch may be greatly aided by closing the eyes and concentrating the mind fully upon the business in hand.

A CASE OF RUPTURE OF THE RIGHT AURICLE OF THE HEART, IN LABOR.

BY S. HUDSON, OF WHITTLESEY, OHIO.

In the March number of the Ohio Medical Journal of 1860, I noticed an extract from the London Lancet, in which was reported a case, in some respects similar to the following:

Mrs. S. M., aged 22, a primapara, was expecting to be confined. I was called to see her December 9th, 1859: found her complaining of a severe pain in the left side, over the region of the heart. The pain was severe: she said she could not draw a long breath. I ordered a foot bath, bled her freely, applied sinapisms over the seat of the pain, and gave her an anodyne; after which she appeared quite comfortable until the evening of the 12th, when I was again summoned to see her in haste. I found her in the first stage of labor; her water had escaped, and to all appearance she was doing well. Her pains were regular, though not severe; and, upon examination, the os uteri was dilated to the size of a silver dollar. A loop of the umbilical cord protruded some two inches or more in length; the parts were moist, the pains regular, and everything, as far as related to the mother, promised a speedy and favorable termination. I endeavored to return the funis, within the cavity of the uterus, but did not succeed. I then stepped into an adjoining room, thinking what I had better do in order to save the child, when my attention was quickly called to my patient by her attendants, who said, in great

alarm, "Come, Doctor, she has fainted!" I stepped to the bed, and found she had thrown herself forward, so that her head was partially off the bed. I had her immediately placed in the horizontal posture, and felt for her pulse, but it was gone; put my ear to her chest, and her heart had ceased to beat! This was all the work of a moment: with a single gasp she had expired. The pains were not so hard but that she continued to laugh and joke, until the last one, when she said, "My pains are growing harder: oh, I shall die!" and died. I told the friends that she was dead—probably died from rupture of some important blood-vessel; perhaps an aneurism of the aorta.

The next question was, what was to be done with the child? It was struggling for life. I told the husband, and mother of the lady, that I could save the child, and that I felt it my duty so to do; but in this I was overruled by a number of very officious old ladies, who soon gathered in, and declared it would be more humane to let the child go with the mother, than to have it brought to the world through such unnatural means as I proposed; and, besides, perhaps the mother had only fainted, and might yet come to life. Being without counsel, and not positive of the occasion of her death, I had to yield; though, I confess, with many unpleasant feelings, for I was confident that I might have saved the child, and that it was my duty to do so.

Through the kindness of the friends I was permitted to have an autopsy on the following day. Dr. Smith, and others, assisted. In opening the chest, the first thing to which our attention was directed was the pericardium, it being filled to its utmost capacity, resembling, in dimensions, a large inflated bladder. From this we took a common sized tin wash-bason full of coagulated blood, and probably a pint or more of serum escaped into the cavity of the chest. The heart was then carefully removed, and, upon examination, the right auricle was found ruptured near the descending vena cava, its texture being very thin and soft, and easily torn—the rupture being sufficiently large for the passage of my thumb. The child presented nothing abnormal, more than a livid appearance—was a beautiful little girl, and ought to have been saved.

A CURIOUS CASE OF MALINGERING—A WOMAN PASSES "BLACK BALLS" FROM HER VAGINA, AND SIMULATES CONVULSIONS, IN ORDER TO GET OPIUM.

(*Reported by* WM. A. BROWN, *Student of Medicine, McConnelsville, Ohio, in the "Ohio Journal."*)

PROGRESS is the order of society, and every day shows us "something new under the sun," Ecclesiastes to the contrary, notwithstanding. But Science has never as yet come up to the consideration of certain *black lumps* which may be passed from a woman's vagina; and no physiologist has, in the widest range of his subject, treated of this periphenomenon we are about to describe—a fact that may be added to the many more which show "there are more things in heaven and earth than are dreamed of in our philosophy."

A pauper had been a charge upon Morgan county for a great many years, much trouble to the authorities, and a great expense to the people. The Infirmary Directors, cognizant of this fact, and of her requiring a great deal of medical attention, having long understood that she labored under a very strange "irregularity" of the uterine function, were anxious to rid themselves of the expense by having her included in the regular fees for attendance. The doctor was called to remove one of the woman's "black balls," but while doing so she had a terrible convulsive fit, which induced him to break his engagement, saying he would not do the business for "a thousand dollars." His successor, however, succeeded much better in treating her "irregularity," adopting the palliative treatment, and employing "opiates," which the poor woman assured him acted like a charm in calming the constitutional irritation attending such a harassing "irregularity" as she was subject to, and completely controlling the distressing convulsions.

The case was become notorious; although little was known in regard to it, yet many physicians had heard of "the woman who passed black balls," and a few were the glad possessors of specimens of these mysterious lumps, but were yet, very naturally, somewhat "skeptical."

Drs. Brown and Little were finally attending physicians, and were anxious to investigate the character of this unheard-of

morbus. An opportunity soon offered; the woman was taken with one of her "spells." He delivered her of a black ball about the size of a goose egg, which was close up to the os uteri, during which procedure the woman assimilated acute pain, and very nearly went into one of her "convulsions." It was impossible for the doctor to resist the belief that she was of the "genus homo" known to the naturalists as "humbugs."

After the operation he yielded to her entreaties, and gave her opium, which she declared "always made her feel better afterwards."

He then examined the ball, which he found to be of the consistence of moderately dry dough, and of a blackish color; he broke it, and discovered in its substance a small piece of *sewing thread*. His suspicions were now confirmed. It was evident there was no physiological function by which thread was secreted; *ergo*, the woman has been simulating a queer disease, in order to obtain opium. Such was the fact. She manufactured the balls out of dough, colored them black with an infusion of maple bark and copperas, then stuffed them into her vagina, sent for the physician to extract them, and plead for opium to quiet the nervous "agitation of such a travail."

She practised this ruse for *ten years*, by which means she had for so long a period obtained happiness by the lump.

In one instance, when called to relieve her, the doctor found a long roll of "composition" lying alongside her thigh, about as long and thick as the forearm, which she said she had brought away herself. "I can always get the long ones away myself," she said, "but the little ones I can't. I can feel *them* coming down, as *cold as mud*. Sometimes the long ones break, then, oh, doctor, I have so much trouble and pain, that it ain't no use trying. It takes a week for my womb to get full; and it's always soft at first, but after awhile the water runs off, and leaves the hard stuff, as cold as mud, and it makes me have fits sometimes."

The amusement now wearing out, the physician told her she would have to remove her own dough babies. She now found that her narcotic supplies were to be cut off by this turn of affairs, and fell upon another method. She sent for the phy-

sician, saying that she had "falling of the womb." The doctor found what she called the "womb" extending half way down the thighs. "Oh, it must be *returned* by all means," said he. So, getting a firm hold of her womb, and bracing his shoulder against her knee, he made a sudden jerk, and away it came! Good heavens! did the man tear away her uterus? He pulled the thing out from under the bedclothes, and examined it. It proved to be a *bullock's rectum*, with one of her long rolls of dough in it, which she had run up into her vagina. "Oh! my God!" she exclaimed, "I'm caught again! To hell with my luck! I'll give it up now. I swear I'll do so no more."

No more trouble was had with her afterwards: she was furnished with a little opium, in diminished doses, and in course of time her bad habit was broken up.

EXTRA-UTERINE PREGNANCY.

BY C. GOODBRAKE, OF CLINTON, ILL.

Dr. C. Goodbrake, of Clinton, Ill., reports for the Chicago Medical Journal an interesting case of extra-uterine pregnancy, in which he operated, but, unfortunately, with a fatal result. After a preliminary description and history of the case, the Doctor proceeds with the following account of the operation:

The patient being under the influence of a mixture, of one part chloroform to four of sulphuric ether, I made an incision in the linea alba, about four inches in length, down to the peritoneum; no hemorrhage occurring, I cut down through it also. I now introduced my hand, after immersing it in the artificial serum, and soon satisfied myself that it was actually a fœtus enveloped in a sac of its own. The sac was found firmly adherent in the right iliac fossa, and to a considerable extent to the parietal peritoneum on the right side. There were no adhesions anteriorly, nor to the intestines, which were all crowded to the left side. This diagnosis was confirmed by Dr. McHugh, who also made a thorough examination. In order to ascertain the condition of the fœtus, a small incision was made in the sac,

when it was found in a pretty good state of preservation; and upon a hurried consultation, it was deemed advisable to remove it, and as much of its sac as practicable.

The incision was now extended upwards as far as the umbilicus, and down to within an inch of the pubes. The incision through the sac was also enlarged, when the fœtus was removed with great difficulty, owing to the strong adhesions between it and the sac. When the fœtus was lifted out, the cord was found to be yet entire, and attached to a very small placenta, of a cartilaginous character, low down in the pelvis. The placenta was located immediately over the space where the sac also adhered to the broad ligament. The uterus was a little enlarged, but otherwise it seemed in a normal condition. The cord, as much of the placenta and sac as could be got away without lacerating the peritoneum, was now removed, the parts carefully sponged, and the incision brought neatly together by the interrupted suture, supported by adhesive strips, and the dressing finished by the compress and bandage. The patient rallied from the effects of the anæsthetic about the time the dressing was completed, and was placed snugly in bed, a dose of laudanum was administered, and she expressed herself as quite comfortable. Her pulse was good.

The time occupied in bringing her under the influence of the anæsthetic, in the operation, and until she was placed in bed, was forty-five minutes, as observed by Mr. Shurtleff. It was estimated that there was not over an ounce of blood lost during the operation.

The patient was left, according to previous arrangement, in the care of Drs. Richards and McHugh, one of them remaining the first night at her house. They visited her regularly afterwards, twice a day. The bladder was evacuated morning and evening, and opium and brandy administered according to indications.

The physicians reported that the patient did quite well for the first forty-eight hours; after which she became restless, her pulse grew gradually weaker and more frequent, until it became imperceptible at the wrist.

Dr. McHugh informed me that the wound looked well, and

that there was no swelling of the abdomen up to the time of her death, which occurred on the fifth day after the operation.

QUININE IN UTERINE HEMORRHAGES.

In the March number of the Charleston Medical Journal and Review, Dr. J. S. Rich, of Georgia, reports several cases of protracted uterine hemorrhage of an alarming character, speedily relieved by the use of quinine, after the failure of all other known means. The following are his favorite methods of administration:

"℞—Sulph. quinine..... ʒj;
Sulph. ferri ..... ʒj;
Mucil. gum. Arabic, q. s. ut. ft.
Pillulæ, No. xxx.

℞—Sulph. quinine..... ʒj;
Sulph. ferri ..... ʒj;
Gum terebinth. ..... ʒj.
M. ft. in mass. et div. in pillulæ, No. xxx. S.—Two to be taken morning noon and night."

One of the Editors of the Lancet and Observer says: "We recently attended a case of uterine hemorrhage, in connection with Professor Richardson, of this city, which assumed a regular intermittent type, and was cured by the free administration of quinine."

[We have found nothing more likely to produce abortion in pregnancy than the administration of large doses of quinine.]

Purulent Discharges of the Mucous Membranes.—Discharges from the mucous membranes are very frequent after eruptive, typhoid or continued fevers of children, and obstinately resist the ordinary modes of treatment. M. Trousseau recommends that these affections should be combatted by lotions and baths containing the sulphuret of potassium (lime of sulphur) in solution.—*Révue Médicale.*

Sunken Nipples.—In the Medical Press, for December 3d, in a lecture upon the "Management of the Puerperal Woman and her Infant, during the Month," Professor Bedford has the following practical remarks: "It will sometimes happen that the nipple is quite sunken and flat, so much so that it will be impossible for the child to grasp it in his mouth. The consequence will be, that the mother is fretted and fatigued by the negative efforts of the infant, and this latter will be defrauded of what it has a birthright claim to—its natural nourishment. It is the easiest thing imaginable to remedy the difficulty. Take an ordinary pint bottle with a long neck, fill it with hot water, then pour out the water, and apply the mouth of the bottle immediately over the nipple: as the bottle cools a vacuum is formed, and thus a powerful but equal suction is produced, which results in elongating the nipple. The bottle is then removed, and the child applied."

Physiology.

PHRENOLOGY.

By V. H. Taliaferro, M.D., of Atlanta, Georgia.

In the January or February issue of the Atlanta Medical and Surgical Journal there appears an article by Joseph W. Clift, upon the above subject, which takes in review our editorial article in the Medical and Literary Weekly of July 2d, upon "Temperament *vs.* Phrenology." He sets out with the assertion that "The writer seems never to have studied Phrenology with an unprejudiced mind, and labors under a misconception of its teachings." Were this charge true, our views in relation to it would doubtless have been more in accordance with those advanced by our critic and reviewer. In reference to this charge of handling that we understood not, we will simply state that we have been the student of ancient and modern phrenologists, the notorious "water-cure doctors," Fowler and Wells, included; and if we are not much mistaken, these great *Hydropathic* lights stand at the head of American Phrenology. Perchance these false dog-

mas would have led us far in the dark, but for the sound practical truths of Dunglison, Dalton, Draper, Paine and Carpenter, whose wise teachings hold up in the clear sunlight of science the palpable errors and deceptions of Phrenological teachings. With these lights, and the guidance in our anatomical demonstrations of that great man, Granville Sharp Patterson, we saw no evidences of truth in Phrenology. Yes, sir, we have studied Phrenology with the same spirit which guides us in the investigation of any and all scientific questions, and most candidly believe it a "speculative theory," coëqual in its ridiculous folly and error with clairvoyance, mesmerism and witchcraft.

In the paragraph following the one in which we "seem" not to have studied, &c., our reviewer says: "Phrenology teaches that as we possess a pair of hands, eyes, ears, &c., so in like manner are our mental faculties double," &c. We have no question that were it not unmistakably perceptible that but one head belongs to every man, it would be stubbornly claimed that as he has two hands and two feet, "so in like manner" must he have two heads. And furthermore, says our reviewer—"His allusion to the 'Flat Heads' is equally unfortunate for his argument. Because development of certain faculties of the mind is always followed by increased nutrition, and certain portions of the brain are enlarged faster than surrounding organs, it does not seem to justify the conclusion he seems to have arrived at, that we, by creating an abnormal growth, could change the faculties." Abnormal means unnatural, or a departure from a given rule or regularity. Now, if an organ be unusually or unnaturally large, it is abnormally developed, whether by a natural or mechanical process. The arm of the blacksmith is abnormally large, and yet its strength is proportionate to its size, and were it in our power to develop muscular structure without the exercise, it would be equally powerful.

Were Phrenology true—every faculty of the mind having its distinct and separate locality upon the brain, and the strength and power of each faculty judged by its corresponding cranial protuberance—then, most assuredly, would the "flat heads" be deficient in all their nobler traits, from the fact of mechanical

obstructions to the growth of the organs, characterizing them, while the more lateral and posterior organs of the brain would be abnormally developed, and in a corresponding proportion, abnormally active and powerful. But the phrenologist would answer these objections to his favored so-called science, by saying that if an organ, or any number of organs, were prevented, by any mechanical means, from development, that the activity of such organs would not be interfered with, because of a corresponding increase in the development of the brain elsewhere. Truly is this a flimsy argument for Phrenology! For instance, were we to make pressure upon the organs of benevolence and veneration, and thereby prevent their growth, other sections of the brain, having different and distinct functions to perform, could not take upon themselves the double office of supplying these deficiencies, because their functions are separately distinct. But holding, on the other hand, that mind can only be made manifest by the action of the brain as a whole, then these difficulties are easily solved, and we are at no loss to understand why it is that the characteristic traits of the Indian are the same, no matter in what shape he may grow his cranium.

Phrenologists, since the days of Gaul and Spurzheim, have based their theory upon the idea of the plurality of the mental faculties, having severally and separately their distinct localities in the brain — each one of these distinct localities having, as it were, independent functions, and being known, as to their size, stregth and power, by developments upon the cranium. These, then, should accordingly be their only guide, but in order the more effectually to elude the force of all arguments against it, they are now claiming, as within the *broad scope* [ha! ha!] of Phrenological science, those general characteristics which belong to different temperaments, different races, and different animals. As well might the "two hands," "two feet," gout included, be termed Phrenological. Physical organization gives peculiar character to temperaments, races and animals. An Irishman is an Irishman, no matter under what sun he claims his birth. An American is the same in general characteristics, though born and reared upon European soil. When phrenologists, by examination

of craniums, will give us the different temperaments and peculiar characteristics of different races, then we will begin to admit that there is truth in Phrenology; but, instead of this, they, as frequently as otherwise, fail to point out the most prominent faculties of the mind, or represent the weakest faculties as being the most prominent and powerful. A reputable and far-famed champion of Phrenology, upon an examination of my own cranium, told me that my *musical* talent was unusually developed —that I could, with little trouble, perform well upon any instrument, and *sing any song*—and, indeed, that music was the great delight of my nature, and that this talent was inherited from my mother, who, without doubt, was exceedingly musical. This announcement very naturally created no little mirth among my acquaintances, knowing, as they did, my inability, from childhood, to be able to sing or whistle correctly the most simple song, or play a tune upon any instrument, notwithstanding my more musically inclined father hitched me regularly to every singing school, fiddle school, or other musical institution which might happen along; and what is still very remarkable, my mother never was known to sing a song so that it could be recognized. This is no isolated case—they are numerous.

We assert, upon physiological authority and the science of accumulated ages, that plurality of mental manifestation, as belonging severally and separately to distinct and particular parts of the brain, is unestablished in the slightest degree. Gaul and his confederates most signally failed, in their dissections of the brain, to elucidate their theory, but, instead, added confusion to uncertainty. The National Institute of France, influenced by the pretensions of Drs. Gaul and Spurzheim, deputed five of its most learned and scientific members to carefully dissect the human brain, in connection with the authors of this new and astounding theory, to ascertain the grounds, if any, for its establishment, and give a "clear and precise abstract of it." The result was, that the report of this committee, far from confirming the views of Gaul, proved a most insurmountable barrier to the adoption of his views, and to-day, Phrenology is almost wholly discarded by scientific men and authors. Dunglison, Carpenter, Dalton,

Draper, Paine — all discard it, upon the ground of a false basis, while the world-renowned quacks, "Water-cure" Fowler and Wells, lead the van of seekers after phrenological wonders.

Were Phrenology true, the National Institute Committee would certainly have found, as they expected, some anatomical divisions or limits to separate parts of the brain, having distinct and different functions. But, alas! for the beautiful theory of Gaul and Spurzheim, no such divisions or limits could be detected, and yet how exactly precise, even to the hundredth part of an inch, is checked off upon the craniums the different faculties of the mind. The layer of gray matter which occupies the surface of the brain, and which ramifies with its beautifully folding convolutions, is continuous throughout each principal portion of the brain.

In this gray matter is supposed to reside the active functions of the brain, bearing the same relation to the other structures of the brain that the ganglions do to the nerve. Now, in nervous ganglia having separate functions, anatomical divisions are always traced; but the surface of the brain we find to be one continuous and compact pulpy mass, having not the minutest trace of dividing lines in its structure. We would as much expect separate distinct functions of the brain traceable by divisions as in nervous ganglia.

Cruvellhier, in his "Anatomy," says — "The depth of the convolutions varies from ten to fourteen lines, but it is extremely variable in different individuals; moreover, there are, perhaps, not two convolutions, nor two parts of the same convolution, which correspond in thickness in the same brain.

"It would be undoubtedly curious to describe minutely all the convolutions. Vesalius, who appears to have entertained the idea of so doing, likened the appearance of the surface of the brain to those irregular forms which are traced by unskilful painters in delineating clouds. Vicq d'Azyr made an unsuccessful attempt to elucidate this subject. Gaul and Spurzheim, who were interested in giving a minute description of each convolution, abandoned the task. I have myself attempted, and so has Rolando, to describe and name some of them." Thus do we perceive, from this standard anatomist, the impossibility of

describing these convolutions, upon the mere superficial surfaces of which, according to Phrenology, are seated separately the organs of the brain. What a brazen absurdity, then, the mapping out, upon these variable convolutions, the upper surfaces of which only can be explored, so *precisely accurate*, every faculty of the brain separately and distinctly, while, indeed, many of these convolutions never present themselves upon the outer surface of the brain, but lie buried beneath other and larger folds. One simple physiological and anatomical question successfully refutes the whole doctrine of Phrenology, viz.: In what part of the brain structure is seated mainly its great function?—*in this gray matter, which makes up the entire surface of the brain.* Is this entire surface subject to external exploration?—*it is not*, and there is the end to the whole of it.

The plurality of the mental faculties must be admitted, but we regard them as the result of the action of all parts of the brain, conjointly and in unison; and in this opinion we are sustained by every physiological and anatomical evidence bearing upon the subject. The manifestations of the mind are subject to many controlling circumstances, and which are not necessarily dependent upon any change in the organization of the brain. Though, from any circumstances, a faculty of the mind be weakened or destroyed, *no corresponding change can be detected in its organ, as recognized by phrenologists.* This is incompatible with all known physiological laws. "How decided an alteration in the disposition is produced by emasculation," and yet without the least change in the organ called amativeness. Now, it is well known that no nerve or tissue of the body loses its function without an evident and decided change in its structure. "Two animals can scarcely be more different from each other than an ox or a bull," and yet, though the psychical manifestations are so changed, the cerebellum* maintains its natural development and proportions. Professor Dunglison, in his Cyclopedia of Practical Medicine, says: "Cretins† in whom the cere-

* According to Gall and Spurzheim, the animal instincts all have their seat in the cerebellum.

† This is the name given to deformed and wretched beings met with in the Pyrennees, Berne and Upper Gascony. In France they are called Caputs.

bellum is defective, display more or less idiotism or defect of intellect, but no corresponding deficiency in the sexual instincts, which, on the contrary, often exists in such unhappy beings in the greatest intensity, and impels them to furious excess. Again, injuries of the posterior part of the head are observed to be followed by stupor and loss of memory, indicating the function of the cerebellum to be connected with the exercise of the mental faculties rather than animal propensities." This is a stubborn fact for Phrenology, and one within itself sufficient to stamp it, for all time to come, as unreliable and untruthful.

We would call upon our reviewer for refuting *physiological facts*—we do not want speculation. When we remember that the convolutions of the brain are from ten to fourteen lines in depth, and therefore the edges only subject to external exploration, beside other equally strong facts, we are irresistibly forced to the conclusion that, not only is it a "speculative theory," but that its teachings are worthy the profoundest contempt of scientific men.

But still there comes a more serious objection to Phrenology, as a science, than has yet been given. All the mental faculties have been located upon that portion of the brain accessible to external exploration. In saying *all*, we mean *all*—every one—not a faculty but has its corresponding *accessible* organ. Now, we emphatically assert, that the *entire surface of the base of the brain*, including the great part of the cerebellum, is inaccessible to *phrenological fingers' tout ensemble*. Not only so, but the inner surfaces of the convolutions, those of the great longitudinal fissure, as well as the surfaces of the fissure of sylvius which divides the cerebrum into an anterior and middle lobe, are equally inaccessible.

These hidden surfaces make up at least two-thirds of the whole surface of the brain, and we would, in all sincerity, ask if it is reasonable to suppose that it has no agency in mental manifestations, because it is inaccessible, and cannot be made to fit phrenological theory.

Putting aside all scientific facts, Phrenology cannot be sus-

tained by outside evidences, as the collection and comparison of crania of individuals of different mental traits.

Professor Dunglison, whose physiological research and contemplation surpasses any of his day, says: "If, for example, we should examine a hundred monomaniacs, in all of whom certain feelings and propensities has been developed, even to morbid excess, and it should be discovered by a person competent to form a judgment on this subject, that no evidence displays itself in the cranidocopy of so many individuals tending to support the doctrine, we should hold that it ought in all fairness to be abandoned. Some hundreds, and even thousands of such persons, have passed their lives under the inspection of M. Esquiral, who possesses most extensive resources for elucidating almost every subject connected with the history of mental diseases, and has neglected no inquiry which could further the attainment of that object. The result of his observation will be allowed to be of some weight on the discussion of this question, in which the appeal is principally to facts of the precise description of those with which he has been chiefly conversant. At his establishment at Ivry he has a large assemblage of crania and casts from the heads of lunatics, collected by him during the long course of his attendance at the Salpetriere and at the Royal Hospital at Charenton, which is under his superintendence. While inspecting this collection, the writer of the present article was assured by M. Esquiral, that the testimony of his experience is entirely adverse to the doctrine of the Phrenologists—it has convinced him that there is no foundation whatever in the facts for the system of correspondences which they lay down between certain measurements of the existence of certain mental endowments. There are few, if any, individuals in Europe whose sphere of observation has been so extensive as that of M. Esquiral."

Phrenology a science! One would doubtless be convinced (?) by the skillful and wonderful manipulations of such mesmerising luminaries as we have recently had among us, who not only mesmerise their subjects, but call in play any mental faculty, by touching its corresponding cranial development. We have as yet

seen only human crania examined, but we suppose that *well-educated* phrenological fingers would very readily decide as to the amiability—kicking or running qualities of a horse—give their color, number of spots, their durability, &c., as well as give the pedigree of chickens, their capacity for laying, &c., also localizing the *fives*, *tens*, *twenty's*, *&c.*, within *iron safes*, by the corresponding developments, depressions, and such like *incontestible* (*?*) evidences.

Our reviewer winds up his defence of Phrenology with the following questions:—1st. "If the brain continually acts as a unit, how and *why* can we exercise several faculties at one and the same time?" 2d. "And why is there such a thing known as a *monomaniac?*" 3d. "And why is Phrenology false, when it teaches that the intellectual faculties of the negro are so small that he is better fitted for manual labor than intellectual effort?" 4th. "And why do we instinctively judge of a stranger's intellect by the shape and general conformation of his head?"

To question first, we *deny* that several faculties of the mind can be called into exercise at one and the same time, else we might translate Eurypides, solve one of Euclid's most difficult problems, and laugh and talk with our friends of past occurrences, "*at one and the same time.*" To question second, we would answer, that "such a thing as a monomaniac" is known; because a man may lose his capacity to control his mind upon a particular subject, which may occur from a variety of circumstances. *Monomania* cannot be established by pathological evidences in the brain, which, in case Phrenology were true, there would be no difficulty in doing. In answer to question third, we would state, that we were not aware that the negro's capacity was judged of by the weight or bulk of his head. If so, we would expect a few Websters looming up from among the unambitious thick-lipped species. We think we could measure some *whappers* in our locality. To question fourth, we would reply, that *we* did not know that *strangers'* intellects were known by the shape of their heads, and were not apprised before that any body else knew it.

ON THE PHRENIC NERVE.

BY PROFESSOR LUSCHKA.

In a monograph by Luschka, on the phrenic nerve, the author arrives at the following conclusions:

1. The phrenic is not merely a motor nerve, but a mixed nerve, containing sensory filaments distributed to the pleura, pericardium, and the peritoneum, covering the diaphragm, and on the anterior wall of the belly. It is also distributed to the coronary and suspensory ligaments of the liver.

2. It brings about a double interchange of fibres between the sympathetic and spinal nerves, since organic nerve-fibres go to it from the inferior and occasionally the middle cervical ganglion, and it gives, by its abdominal portion, fibres to the solar plexus.

3. In the majority of cases the phrenic arises but from one cervical nerve—the fourth.

4. The diaphragmatic branches he traces to the tendinous centre, the inferior vena cava, the right auricle, and the liver.

5. In its course over the pericardium it appears to be endangered in diseases of the pleura and lungs, especially tubercular. Hence, probably, some of the disturbances of respiration in these complaints. —*Schmidt's Jahrbuch.* (*Med. Chir. Rev.*)

ON THE FATTY MATTERS OF THE ECONOMY.

BY M. DE QUATREFAGES.

At a recent meeting of the French Academy of Sciences, a report was read, by M. de Quartrefages, upon a memoir of MM. Lecase-Duthiers and Leriche, entitled, "Researches upon the Alimentation of the Gall-Insect," which was intended to throw some light upon an important physiological question—the origin of fat.

It is well known that on this subject there are two hypotheses generally professed by physiologists. According to some, fatty matters are always of vegetable origin. They are already formed in vegetables, and are only absorbed by animals, who destroy them by oxidation. Others contend that fatty principles may

also have an animal origin, and may be formed in the economy by transformation of alimentary substances, whatever the composition of the latter.

Chemists have not hitherto been able to solve this problem. In order to do so, it was necessary to analyze the whole amount of food, and subsequently the tissues formed from this food. Experiments having been usually practised on large vertebrate animals, such a process was out of the question. MM. Lecaze and Leriche have therefore experimented upon the *cynips quercûsfolii*. Having studied the structure of galls, M. Lecaze observed that they were composed of concentric layers enveloping an alimentary mass, in the centre of which the gall-insect deposits its eggs. This mass, consisting chiefly of starch, is isolated completely. M. Lecaze then associated himself with M. Leriche, in order that his results might be verified by chemistry.

These two collaborators analyzed the alimentary mass and then the entire tissues of the gall-insect, and demonstrated that, in the cynips, under the influence of animal life, the elements of starch served in part in the fabrication of fatty matters. They proved, further, that the nitrogen which exists in the food of this insect is almost entirely consumed in the formation of its tissues.

Spleen, Tumor of.—In a case of splenic tumor, Dr. Gurtrac (L'Un. Méd.) has employed the sulphate of manganese with good effect, in doses of one to one and a half grains twice a day, in the form of a pill.

Many of the uses to which compressed sponge has recently been applied were practised during the last century. The following directions for preparing sponge for dilating sinuses is given in an old volume by John Jones, M. B., published in 1797: "Dip it in water, wring it out, and then wind it about with packthread as tight as possible: the sponge then to be cut off as wanted, letting the thread remain on the rest of it."

Bibliographical Notices.

Transactions of the American Medical Association, Vol. XII. 1859.

We have at length seen a volume of this work, though too late for an extended notice of it in this number. It is true, that we knew of its existence some time since, through the various journals of the country; yet, as the Publishing Committee did not recognize us as one of the "standing Periodicals," and we were determined to stand upon our dignity in this regard, we should not have been blessed with a sight of this "literary curiosity," but for the courtesy of a friend. The National Association is certainly a great institution *per se*, but we cannot help expressing the conviction that the books which are annually produced under its auspices do not properly represent the real scientific status of the profession of the United States. And the deficiencies of these productions can be easily accounted for when we remember the manner in which committees are appointed, and the complete legislative monomania which seizes upon the members generally, to the annihilation of all other considerations,—together with the total irresponsibility of those to whose hands are consigned the important task of publishing the proceedings of the Association. We trust, however, that the resolution by which the Association has been divided into sections,—each devoted to some special scientific subject,—will revolutionize that body itself, and effect a most important and salutary improvement in the works which it puts forth as its "Transactions."

The paper of Dr. Joseph Jones,—which we are pleased to see was not published as a Prize Essay, the instruction of the Association to the contrary notwithstanding,—is the longest, and perhaps the ablest in the volume. Its author certainly shows himself a most enthusiastic and untiring student, and has collected an immense amount of valuable information in regard to the subject of which he treats; but we would be false to our trust as a public journalist did we not say, that in many instances both his inductions and deductions are incorrect.

In some respects the Louisville Meeting was a decided success; for, both as regards the numbers present, and the manner in which they were entertained, it has not been surpassed by any other in the history of the Association.

A Practical Treatise on Fractures and Dislocations. *By* Frank Hastings Hamilton, M.D., *Professor of Surgery in the University of Buffalo, &c.* Philadelphia: Blanchard & Lea. 1860.

We regard this as incomparably the best treatise on Dislocations and Fractures that has ever been published, and we can but feel proud of the fact that it is the production of a compatriot. We hardly think that hereafter the question can be asked, "Who reads an American book?" for the present work must be favorably received throughout the whole medical world,—that is, by those who are capable of appreciating high scientific merit, and true professional skill. The American mind is not only essentially active, but preëminently practical, and in nothing, perhaps, has it displayed this particular combination of qualities more decidedly, than in that department of Surgery to which this work is directed. To all who desire a work thoroughly up to the times, either as a text-book, or a guide in the practice of surgery, we recommend this work in the most emphatic and unqualified terms.

Therapeutics and Materia Medica. *A systematic Treatise on the Action and Uses of Medicinal Agents, including their Description and History. By* Alfred Stillé, M.D. Philadelphia: Blanchard & Lea. 1860.

This is a great book—one which will place Dr. Stillé in the front rank of medical authorship. Its style is graceful, yet as strong and manly as the most fastidious could desire; its classifications are simple, but at the same time accurate and natural; its explanations of the action of remedies, though based upon the most modern and approved pathological principles, are so plain and satisfactory as to be universally acceptable; whilst, as a treatise on Materia Medica proper, it has no superior in any language. We know that this may be esteemed the language of hyperbole, or, perhaps, of flattery itself, but as it expresses our deliberate judgment, we are prepared to stand by every word of

it, whatever may be the opinions of others. Hereafter we shall notice it more in detail, whilst for the present it will suffice to offer our congratulations, not only to the Author for the distinguished credit which he done himself in this regard, but to the whole medical profession upon the opening so rich a mine of scientific truth.

Clinical Lectures on certain Acute Diseases. *By* Robert Bentley Todd, *M.D., F.R.S., &c.* Blanchard & Lea: 1860.

Dr. Todd is certainly one of the ablest of the many great men who adorn the Medical Profession at the present time; and yet he has his peculiar whims, just like any other genius. This is particularly well exemplified in the volume before us; for though it contains much that is valuable to the clinician, and worthy the careful study of every student of medical science, it promulgates doctrines which are entirely too chimerical and ultra to receive the general approbation of the Profession. The subordinate position which he assigns to mercury, as a therapeutical agent, is a fair illustration of the truth of this remark; for, though that remedy may have been invested with too much importance by a large majority of the Profession, in the treatment of inflammation and its products, the assertion of its complete impotence in this connection, is assuredly incorrect in itself, because opposed by the daily experience of every candid practitioner of Medicine.

We are likewise utterly opposed to the principle embodied in the assertion "that the notion so long prevalent in the schools, that acute disease can be prevented or *cured* by means which depress and reduce the vital power, is fallacious." This is Rationalism run mad. It is the very consummation of medical skepticism. It is the utter repudiation of the wisest and most important therapeutical principles known to the science of Physic. It is the virtual rejection of some of the most valuable agents of the Materia Medica, and the curtailment of that department of Medicine within the most circumscribed and unnatural limits. And although invested with all the *prestige* which the names of Bennett, Forbes, Todd, &c., can command, the Profession recognizes it as nothing more or less than the discarded system of

Thomas Brown, arrayed in more modern paraphernalia, and rejects it as insufficient and effete.

King Alcohol has had multitudes of votaries, from the birth of Bacchus to the distillation of whisky; but not until these latter times was there a deliberate attempt made to enlist the disciples of Æsculapius under its banners, and to demand that even Science itself should do it reverence. To Dr. Todd, the Ajax Telemon of the profession,—the most distinguished of England's illuminati, the honor belongs of having placed Alcohol upon the very pinnacle of modern therapy,—of having elevated it to the most commanding and important position among remedial agents,—and of having discovered in it the veritable panacea for "the ills which flesh is heir to," whether acute, chronic, or what not. In the work before us we find alcohol recommended when there is fever and no fever,—in delirium and out of it,—in coma and in wakefulness,—in anæsthesia, and in hyper-anæsthesia,—when the pulse is full, and when it is feeble —and, indeed, whenever a mouth or a rectum can be found to receive it, whatever the name or nature of the existing malady. Now, though "stimulation" may be the treatment for patients generally, and hospital patients particularly, the physician who attempts it in the universal and indiscriminate manner advised by Dr. Todd, will soon find, that he has been making his victims *merry* at the expense of his reputation, and that, whilst pouring ardent spirits down their throats, their own spirits have quietly taken out for parts unknown.

Though the book thus abounds in false theories and illogical deduction, it nevertheless contains much that is valuable in pathology and therapeutics, and is well worthy an attentive perusal.

We are indebted to Messrs. Blanchard & Lea for several other very valuable works, which shall be duly noticed.

Editorials and Miscellaneous.

THE ANNUAL MEETING OF THE STATE SOCIETY.

It is not a very difficult thing to write the history of the Meeting at Washington, for the simple reason, that there is but little to write about. It is true that the various sections of the State were well represented,—that some of the ablest of our physicians were present,—that the utmost harmony prevailed among the members generally, and that the Society was entertained in a manner which did credit to Carolina hospitality, and all honor to the noble-hearted citizens of Washington; but, as far as the legitimate business of the Society was concerned,—as regards all matters pertaining to Medicine,—in relation to those great subjects in which professional men are particularly interested,—as a scientific Convention proper, the Meeting was not what it should have been, or what future meetings will be, we are certain. These facts are to be deplored, it is true, but they are far from deserving the significance which some attach to them, for obvious reasons. Associations, like the individuals who compose them, have their periods of activity and repose,—their working and their idle hours,—their excitements and their relaxations, the one succeeding the other so naturally and uniformly as to establish a reciprocal relation between them, which is in itself an evidence of the vigor and health of the organization. When such a body as ours has been earnestly laboring for years in the great work of Professional advancement,—when all its energies have been called into exercise in the effort to secure such educational reforms as the best interests of Medicine imperatively demand,—when it has been successfully engaged in establishing and sustaining an enterprise which is preëminently calculated to foster the talent of the State, and give prominence to her citizens,—it is not unnatural that the Society should feel disposed "to rest from its labors," and to prefer the pleasures of relaxation to the sterner demands of duty. It had labored hard—it required rest—and it took a holiday. It had grown weary of the harness, and kicked it off for a little sport. It had become

sick of "the shop," and physic, and it resolved on "sinking" them both for the nonce. This is, we believe, the proper explanation of what some malevolent croaker may be disposed to denominate the "Washington failure"—for there are inveterate grumblers in the Profession,—and so regarding it, we can but esteem the perfect *abandon* of the occasion, rather as the exponent of that conscious power which redundant vitality inspires, than as a harbinger of incipient weakness and coming death.

It is but fair to confess, however, that had the Society been in the soberest of moods, and on the most serious work intent, the difficulty of remaining in such a *status* would have been particularly great in Washington,—not that its people are inimical to the Profession, or uninterested in scientific investigations, but because of the sundry temptations to complete obliviousness of all serious matters which continually presented themselves in the beautiful women and princely hospitality of that ancient town.

It should not be supposed from all this, that nothing was done in Washington, for our previous remarks are to be taken in a qualified and comparative sense.

The election of Doctors C. E. Johnson and S. S. Satchwell, to edit the Medical Journal, in the place of the present editor, who will reside in Baltimore after the conclusion of the present volume, was a matter of much importance in itself, since it evinced a determination to sustain an enterprise with which are identified the highest interests of the Profession of the State, and which, we are proud to say, has already given a position to Carolina, in the scientific world, that secures for her the profound respect and confidence of her peers.

Then, again, the Address of Dr. W. T. Howard was one of the most beautiful, able and eloquent ever delivered before our Society or any other, and is not only well calculated to enhance the reputation of its author as one of the most finished and erudite scholars in the country, but will reflect the highest honor upon the Association which produced it.

There was another matter, also, in which the Society acted well and wisely—in such a manner, in fact, as must elicit the

admiration both of friend and foe, and at the same time vastly extend the sphere of its usefulness: we allude to the course it thought proper to pursue in regard to Dr. Hill, of Goldsboro. That gentleman had been arraigned at Statesville for unprofessional conduct, and cited to appear at Washington to show cause why sentence of expulsion should not be passed upon him for the professional improprieties of which he had been been guilty.

When the case was called up, Dr. Hill, through a friend, acknowledged his offence,—declared that he had committed the breach of decorum ascribed to him, without improper intentions,—apologized to the Society for his conduct,—and promised amendment. All proceedings against him were then quashed by a unanimous vote of the Association, and the social and professional ban which had been placed upon him by the previous action of the Society, was most promptly and properly removed. We deem it but an act of justice to Doctor Hill, to say, that his conduct, in this connection, was high-toned and gentlemanly, and that it secured the warmest sympathy of every member of the Society. In view of all the facts of this case, we can but rejoice at its termination, inasmuch as the dignity of the Association was fully vindicated, whilst the individual who had rendered himself obnoxious to its censure was relieved from the odium which a persistent violation of its established code would have brought upon him.

The Society likewise considered the propriety of amending the Medical Bill, and though it was universally admitted that, in some minor points, certain alterations are much needed, it was deemed unwise to approach the Legislature on the subject for the present. This we regard as particularly sensible, for the reason, that though the bill has defects, they are not fatal ones by any means, and it is far better to submit to them, for a time at least, than to jeopardize the whole measure by recommitting it to the hands of those who are really responsible for its deficiencies. The Bill is already working admirably, and it is wise to follow the advice of the old proverb, and to "let well enough alone."

To Drs. Hall, of Greensboro, and Tayloe, of Washington,

who alone, of all the members present, submitted reports on scientific subjects—and very interesting ones too, as we expect to prove to our readers—the Association should feel under special obligations. Their example is well worthy of imitation, and we trust that it will have many followers at Morganton, as nothing can be more important to the Society than the consideration of matters of such a character.

The following is the List of Officers elected for the ensuing year, and who, according to the new provision of the constitution, are not reëligible.

Dr. N. J. PITTMAN.....*President.*
" J. J. SUMMERELL, *First Vice President.*
" C. T. MURPHY.....*Second* "
" G. W. HODGES.....*Third* "
" W. A. NORCOM.....*Fourth* "
" W. G. THOMAS.....*Recording Secretary.*
" S. S. SATCHWELL...*Corresponding Secretary.*
" C. W. GRAHAM.....*Treasurer.*
" DAVID TAYLOE.....*Orator.*

But one of the Prizes of the Society was contended for, viz., that of Dr. Alexander, "On the Use of Tobacco, &c."; but as the committee were not prepared to report to the Society, they were instructed to do so through the next number of the Journal. We had hoped that these Prizes would be more generally sought after, and that the competition for them would call out the best talent of the State. Our brethren must be up and doing, if they expect honors and rewards in the Medical Profession. There is no such word as "sloth" in the vocabulary of him who aspires to distinction. The most learned philologists have discovered no truer meaning to the word "genius," than "labor—untiring labor." And we can but hope, that should such Prizes be offered again by the Association, the truth of these remarks will be remembered, and that numbers will enter the lists, prepared to struggle for them in that spirit of honest emulation which has already accomplished such important results for every department of Science.

There were about seventy members present, representing the various sections of the State, and composed of "the hoary-headed, the matured and the young," all of whom enjoyed themselves in such a manner as will fill them with the liveliest

and longest recollections of the citizens of Washington, and their magnificent hospitality.

The splendid ball given to the Association after the adjournment, was one of the principal features of the meeting, and the *fairest* too, if the presence of some of the most beautiful and attractive women of the State could make it so.

The next meeting of the Society will be held in Morganton, where we hope to meet with a still larger number of our professional brethren, for, though resident in another State at that time, we shall assuredly be there.

CANDID!

In order to prevent all misunderstanding, we desire to state, that this Journal will be under our exclusive management until the end of the present volume, and that it will be published in Edenton, as heretofore, up to the first of October, 1860.

In the mean time, we must ask our subscribers, as an act of simple justice, to remit the amount of their subscription to us, since "the laborer is worthy of his hire" under all circumstances, and particularly so when engaged in the onerous task of conducting a Medical Journal. Having accepted a Professorship in the University of Maryland, we shall be necessitated to remove from Carolina upon the conclusion of our present labors, and it is a matter of no little importance to us that all the accounts of the Journal should be settled up at an early day. Trusting that our friends generally will appreciate our position in this regard, we commit the whole matter to their hands, in the confident assurance that the fullest justice will be accorded. Some of our cotemporaries indulge in what they are pleased to call "Hints to Subscribers," when in want of funds. We prefer to speak out plainly, and to let the consequences take care of themselves: *there is money due us, and we want it.*

Dr. Winship, of Boston, the modern athlete, is said to be able to raise the enormous amount of 1230 pounds with his hands.

A PLEA FOR MEDICINE.

In a previous number of this Journal, we indirectly promised to attempt, at some future time, a defense of Medicine; — not that the attacks of its enemies had created a necessity for the performance of such a task, but rather as "a labor of love" on our part, and with the hope of inspiring its votaries with a greater zeal and enthusiasm for their sacred calling.

The principal difficulty in the way of a redemption of this promise, consists in the fact, that a defense of Medicine involves the consideration of such a number and variety of topics, as utterly precludes its proper performance in the contracted limits assigned to the Editorial department of a Medical Journal.

Believing that veritable *progression* is the surest test of the truth of any science, and assured that the history of the various departments of Medicine furnishes the grandest and most conspicuous illustration of this principle, we should be constrained, in justice to the subject, to chronicle in detail the multitudinous facts by which their respective provinces have been beautified and enlarged through the labors of their devoted disciples. This, though a glorious task for one imbued with the true spirit of his noble profession, would be a Herculean labor in itself,— a work requiring the most faithful and protracted research, and demanding, for its proper execution, the exclusive dedication of an entire volume.

But though prevented by such a consideration as this from the performance of the task assumed, we beg leave to direct the attention of our readers to the subjoined argument, believing that it shows that in one point, at least, the truth of Medicine receives the fullest and most satisfactory confirmation.

Though ingratitude has been the reproach of humanity from creation, that exalted sentiment which stands in direct opposition to it,—that noble and admirable virtue which is its antipode, does sometimes find its way into the heart of man in despite of every opposing circumstance, biasing his judgment, mollifying his prejudices, and elevating his entire character. But gratitude is engendered only by a sense of favors actually conferred — by

a conviction that some benefit of a positive and appreciable nature has been received; and when this sentiment has been inspired by any particular system, it is fair to suppose that it (the system) has proved of practical advantage to those thus affected,—has benefitted them in some decided manner. By gratitude, as thus employed, is meant,—not that idle and capricious fancy—those blind and evanescent impulses which invest every possible system to which the charm of novelty attaches itself, with a wonderful but most fictitious and transient popularity,—but that substantial emotion, that abiding principle, that matured sentiment, which, as the legitimate offspring of an enlightened, deliberate and candid judgment, affiliates with all that is best, truest and most enduring in the breast of man, and thus becomes an essential element of his nature. If, then, mankind, after a protracted, deliberate and impartial scrutiny of the claims of Medicine to confidence and respect, really entertains towards it a genuine sentiment of gratitude, the inference is a necessary one, that it has accomplished its noble mission,—that it has secured that "protection and preservation" so ardently desired by every child of mortality, and that it is adapted, in all its fullness and beauty, to the wants of humanity. But, whatever supplies a natural want—a necessity created and continued by the Supreme Architect himself, must be a part of his great governmental policy—of the machinery of the universe—of that mighty system whose every element is an embodiment and illustration of truth in all its majesty, beauty and immutability.

Let us, then, glance at the history of the world, for the purpose of ascertaining whether men have given any practical proofs of their gratitude towards our noble science.

In the earlier ages of the world Medicine was regarded either as a mystery or an imposition. The calling was esteemed a servile or a dishonorable one. Surgery was for centuries in the hands of the barbers; Physic was the peculiar prerogative of slaves and mendicants; the collateral sciences were surrendered to alchymists, priests and jugglers; whilst the incantations of magic, or the unmeaning rites of a fanatical religion, were appealed to by prince and peasant when disease overwhelmed him,

and the grave opened for a victim. But what a different picture meets the eye of the philanthropist of the present day! In every city of the civilized globe the benefits of Medicine are made accessible to all classes of the population by the erection of institutions wherein professional services are rendered gratuitously, and which, by universal consent, are esteemed the crowning glory of the nineteenth century. Multitudes of temples, of unsurpassed beauty and magnificence, have been dedicated to Æsculapius,—whose courts are continually thronged with ardent votaries, and whose glowing altars drink the rich libations of the warmest hearts and the noblest intellects. Not an army marches on its career of blood and conquest without a full complement of medical men, whose services are recognized by a generous acknowledgment of rank, and a liberal allowance of money. Not a ship over which floats a national flag, or wherein passengers are transported, spreads her white wings to the breezes of the ocean, whose decks do not echo to the tread of one who, though bearing only the insignia of our noble calling, is honored in the hour of pestilence or the fight as a hero, or, it may be, a martyr. Not a monarch is inaugurated upon his throne, but pays his homage to our Profession, by selecting some special medical adviser, whose position is one of as much honor and consideration as that of the proudest noble, "with all the blood of all the Howards" in his veins. And not a pestilence stalks through the land, filling it with aching hearts and new-made graves, but skepticism immediately gives place to the most active faith, and the proudest, as well as the humblest, will hang upon the slightest whisper of the "good" physician, as if his were the voice of destiny, trusting to his skill for "protection and preservation," and ready to offer the most tearful sympathy when the destroyer turns upon his pursuer, and adds another recruit to the "noble army of martyrs."

In view of all these practical proofs of the progress which Medicine has made, both in the confidence and regard of men, it cannot be denied that it has engendered in their hearts a deep and abiding sense of *gratitude;* and hence, though the spirit of skepticism is abroad in the land,—though traitorous doubts

have shaken the faith of Humanity until it is only "a rattling skeleton,"—though Reason has, with despotic will, demanded the surrender of much which is dearest and most sacred, the warmest, truest, and most enduring affections of the human heart still cluster around the time-honored principles of Medicine, protecting them against the ruthless Vandalism of the age.

Medicine, then, has engendered a genuine sentiment of *gratitude* in the breasts of men; and it must follow, from our premises, that it is applicable to the wants of the race,—that it does meet and supply a prcördained necessity,—that it is a part of the Divine plan instituted for the harmonious government of Creation,—and that it is *ipso facto* a truth—a science.

We understand that the Medical Board has been in session for several days, and that the following gentlemen have been examined and duly licensed:—

Drs. L. P. Warren, Edenton; W. A. Blount, Washington; J. P. McCombs, Charlotte; J. W. Shinn, Rowan; W. W. McKinzie, Salisbury; L. W. Robinson, Sampson; W. A. Dunn, Wake; H. H. Harris, Wake; J. H. Leary, Bertie; R. S. Petway, Edgecombe; W. H. Lilly, Richmond; J. W. McGee, Kenansville; W. D. Somers, New Hanover; P. F. de St. Clair, New Hanover; C. G. Cox, Onslow; G. N. Ennet, Onslow; D. W. Shaw, Moore.

This practical working of the law must be particularly gratifying to its friends throughout the State, as it demonstrates that their labors have not been in vain, and that the Profession has really a most efficient safeguard thrown around it by this admirable measure. It is sincerely to be hoped that none of our young graduates will be afraid to submit to the ordeal thus imposed, for that fact would prove a greater incubus upon their professional career than a positive rejection by the Board, because it will be universally esteemed a voluntary confession of ignorance and inefficiency. Should there be any one too proud of his diploma to stoop (?) to this compliance with the requirements of the law, we beg leave to quote the following passage for his benefit:

"*Hamlet.*—Is not parchment made of sheep's skin?

"*Horatio.*—Ay, my lord, and of calves' skin, too.

"*Hamlet.*—They are but sheep and calves which seek out their assurance in that."—Shakspeare.

ITEMS.

The wise men of Europe are projecting a Universal Scientific Congress, to be composed of the savans of all the world. The place of meeting has not been decided upon, and it may be that one of our large cities may come in for the honor. Should it come off on this side of the water, we suggest that the Scott County Society, of Iowa, appoint a committee to look out for "self-constituted" delegates.

Dr. Henry F. Campbell recommends the tying of the strands of hair across the line of the cut, in wounds of the scalp, as a substitute for adhesive straps, &c. This is both ingenious and novel, in our opinion.

The New Hospital for the Paralytic and Epileptic was opened in London last month, under the following management: Dr. E. Brown-Séquard and Dr. J. S. Ramskill, Physicians; Wm. Ferguson, Esq., F.R.S., Consulting Surgeon, and J. Z. Lawrence, Esq., M.B.D., F.R.C.S., Surgeon.

At a Meeting of the Association of Physicians of the Department of the Sonne, France, the following resolutions were adopted:

1. "That in charging, the number of visits is not to be considered as sufficient estimation of value for services rendered, but that the gravity of the disease, the importance of the operation, the dangers incurred by the physician, and still other circumstances, such as the social position and fortune, &c., of the patient, should be taken into consideration.

2. "That the period has arrived when physicians should be better remunerated."*

* In response to numerous applications for the Tariff of Charges adopted by the State Society of North Carolina, we have to say, that none has as yet been agreed upon.

Dr. John O'Reilly, of New York, offers two prizes of $250 each, to medical students generally, for the best Essays on the Anatomy and Physiology of the Animal and Organic Nervous Systems. Essays must be handed in, one on March 1st, 1861, and the other on March 1st, 1862.

The Fifth Decennial Convention for the purpose of revising the Pharmacopœia of the United States met at Washington on Wednesday, the 2d ult. Dr. George B. Wood, President of the Convention of 1850, was called to the Chair, and Dr. John C. Riley, of Washington, appointed Secretary *pro tem.*

The following permanent officers of the Convention were then elected:

President—Dr. George B. Wood, of Philadelphia.

Vice-Presidents—Dr. Jacob Bigelow, of Boston, and Dr. Edward Warren, of North Carolina.

Secretary—Dr. Thomas Miller, of Washington.

Assistant Secretary—Dr. John C. Riley, of Washington.

The delegates of several medical bodies, represented in the Convention, were then called on for contributions towards a revision of the National Pharmacopœia, when reports were handed in from the Massachusetts Medical Society, by Drs. Cutter and Bigelow; from the New York City Medical Society, by the same; from the New York College of Pharmacy, by William Hegeman, and from the Philadelphia College of Pharmacy, by William Proctor, junior.

These reports were referred to a committee, with directions to report a plan for the revision and publication of the Pharmacopœia; the committee to consist of Dr. Franklin Bache, Dr. Edward Parrish, Alpheus P. Sharp, Dr. Thomas Miller, and George W. Russell, of Connecticut.

It is now proposed to protect young trees against attacks of rabbits, mice, &c., by rubbing them, from the ground upwards to eighteen inches, with a piece of raw, bloody meat, or painting them with the blood of some animal. It should be done late in the Fall or beginning of winter.

Dr. Horace Green has resigned the Presidency of the New York Medical College, and Doctors Peaslee and Flint have resigned their Professorships in the same Institution.

Dr. Seroc, in one of the late sessions of the Academie de Médécine of Paris, read an elaborate paper on the treatment of Childbed Fever with Digataline.

From the statistics of over 1000 persons of different occupations, who died, within the last 100 or 150 years, at an age of at least 80 years, collected by Faber, and communicated to the Würtemburg Correspondenzblatt, we learn that among these long-lived persons there were—

1. Authors and learned without special profession, 86.
2. Statesmen and diplomatists, 96.
3. Clergymen and theologians of all beliefs, 150.
4. Artists, 167; painters, engravers and sculptors, 72; poets, 42; musicians and composers, 26; actors, dancers and circus riders, 9 (among which were Franconi at 98, Noverre 105, and his two sons at 82 and 83).
5. Military, 190 (army 159, navy 31).
6. Naturalists and physicians, 192; (naturalists and physicians not in practice, 58; practising physicians, 134, of whom there were 3 over 100, 15 between 90 and 100, and 116 between 80 and 90 years old at their death).

Now, though we do not accord to these statistics completeness in themselves, nor regard them as proper foundation for generalizations, we may surely draw from the above combination of ages the satisfactory result that the duration of life among physicians is by no means as limited as so many authors affirm.

The Electric Light has recently been made to subserve surgical purposes, in the city of Paris, by M. Dumoncil. The advantage of illuminating those localities where the admission of ordinary light is impossible, in the performance of such delicate operations as are usually demanded therein, must be appreciated by all.

Appointment in the University of Pennsylvania.—Doctor William Pepper has been elected Professor of the Theory and Practice of Medicine in the University of Pennsylvania, in place of Dr. George B. Wood, resigned.

Dr. Pepper was for a long time known to the profession of this country, as a clinical teacher in the Pennsylvania Hospital, and is one of the most eminent practitioners in Philadelphia. He will bring to his new position practical qualities which will insure him efficiency and popularity. The appointment seemed to be generally anticipated, and is satisfactory to the friends of the institution.

Chloroform in Midwifery.—Dr. Kidd states that in 1699 recorded deliveries in which chloroform was administered, "there was not the slightest accident or casualty traceable to the anæsthetic," and that, in obstetrical practice, there has not yet been one well-authenticated death from it.

A writer in the London Medical Times says, that Mr. Simpson uses annually, in his practice, no less than from five to seven gallons of chloroform.

The Paris Hospital Medical Society offers a prize of 2000 francs to the author of the best memoir on any subject of Clinical Medicine or Applied Therapeutics, the choice being left to competitors. The essays must be written in French or Latin, and be sent in before December 31st, 1861.

Quantity of Nicotine in different kinds of Tobacco.—According to the Boston Medical Journal, the quantity of nicotine in tobacco varies much, according to the locality from whence it is taken. From Havana tobacco, 2 per cent. is obtained, and a like quantity from that of Maryland and Virginia; from Alsace tobacco, 3 per cent.; from tobacco du Nord, 6 per cent., and 8 per cent. from tobacco du Lot and from Algeria. Hence, it seems that the cheapest tobacco contains the greatest quantities of irritating matter; and this is, it is said, an explanation of the frequency of cancers of the lip in the southern districts of France.

Removal of the Tongue.—Dr. Choppin reports, in the New Orleans Medical News, the successful removal of the tongue, in a case of cancer, with the ecraseur. The operation lasted fifteen minutes, and was performed without hemorrhage. This is one of the kind of cases to which the ecraseur is admirably adapted.

There is, in England, an excess of 800,000 females over males. This disparity is caused by the wars and the standing armies, by the drain of men for the mercantile and naval marine, and by the greater number of males who emigrate to the colonies and to the United States.

An exchange says that "a good many of our physicians should be indicted for fast driving,—as they drive to kill."

DELAY!

June 20th, 1860.

Alas, for the vanity of human expectations! Just as we had reached this point in the preparation of the May number of this Journal, and were anticipating its speedy appearance, our Publisher was taken suddenly and most dangerously sick. As it was impossible to procure a substitute, and the attack proved a most obstinate and protracted one, the Journal has been unavoidably delayed until the present time. We regret this unfortunate occurrence, but, as it was utterly beyond our control, we trust that our readers will not feel disposed to censure us for what was really a "visitation of Providence."

We beg leave to refer our readers to the advertisements of the Richmond Medical College, and of the University of Maryland, which will be found at the conclusion of this number. Those who may be disposed to patronize them, will find that Medicine can be as well taught at the South as at the North, and that no pains will be spared to render their courses as attractive as those of any schools in the country.

On the Administration of Medicines to Children.—M. Wahu having much to do with the diseases of children, wishes to impress upon practitioners the importance in the case of important medicinal substances, having them administered, whenever practicable, in their own presence. He also states some of the means he adopts to enable certain medicines to be more easily got down. Thus, the subnitrate of bismuth, in large doses, which is of such value in diarrhœa and the gastro-intestinal affection of children, often subsides to the bottom of the spoon when given in broth or milk; and it is much more readily taken also by children of about two years old, when given in chocolate prepared with water and thickened with tapioca or crumbs of bread. In this way seventy grains and more may be given night and morning. In the same way iron can be very readily given. Rhatany and catechu, two precious drugs, the action of which, when in small doses, is soon manifested in children, can also very readily be given in this chocolate panada. Chocolate made with either milk or water, and flavored with canella or vanilla, is usually very readily taken by children, and its dark color facilitates the mixing of numerous colored medicinal substances, which would be observed by the little patients were they given in milk, broth, or any infusion. Rhatany and catechu may also be well triturated and mixed with quince jelly, the flavor of which, while masking that of other substances, is very agreeable to children. Sulphate of soda and sulphate of magnesia are substances very difficult to get even adults to take. The sulphate of soda may often be administered to children, by dissolving 10 parts in 150 of unsalted beef tea, and waiting until the child is sufficiently thirsty to swallow a cup of liquid almost without tasting it. For adults, the best means is to dissolve the sulphate of soda or magnesia in exactly the quantity of hot water necessary for its complete solution. This is allowed to get cold, and a glass of pretty strong lemonade is prepared. Holding a glass in each hand, that containing the salt is rapidly drank, and then the lemonade is slowly drank—masking the detestable taste of the purgative, and supplying enough fluid to prevent its proving too irritating. Corsican moss is another substance which children take with

difficulty; but if an infusion be made and strained, and then added to unsalted beef tea, it will be readily swallowed. Calomel is one of the most difficult medicines to give, when children are too young to swallow pills, which is the case under six years of age. Incorporating it in honey is the best means—rinsing the mouth afterwards to prevent any adhering to the gums. It should never be given in currant or any other jelly, a death having occurred, a few years since, from the conversion of the calomel given in currant jelly into a bichloride. It is safest to prohibit any acid drink being taken on the day that calomel is given. Ipecac. may be given either in the chocolate panada or in honey. When it is impossible to give any medicinal substance by the mouth, it may be administered by the rectum, taking care, first, to empty the gut by tepid water or an emollient decoction, and that the bulk of the medicated enema do not exceed from four to six ounces, so that it may be retained and absorbed. It should also be borne in mind that as some substances, such as opium and other narcotics, are easily absorbed by the large intestine, doses less than would be given by the mouth must be employed. Quinine, on the contrary, requires to be doubled in quantity to be of any use. In forcing a child to swallow, the usual practice is to pinch the nose; but a part of the medicine will be rejected, if precaution be not taken to keep the nares completely closed *until deglutition is completed.* So, too, when medicines are thus forced down a child, it must be kept the whole time in a *sitting posture* until deglutition has been completely accomplished. A suffocative cough may be induced, if it be allowed to lie down again still having some of the liquid in the mouth.

—*Med. Times and Gaz.—Louisville Med. Journal.*

The Union Médicale reports a case of *fracture* of the skull in Natural Parturition. The woman was three days in labor, and was delivered without instruments, after great efforts on her part.

It was the unanimous opinion of the ladies of Washington, that the State Medical Society was composed of the best-looking men that had ever been seen in the town.

THE EVILS ARISING FROM TIGHT LACING form the subject of an article of Dr. S. C. Young, in the New Orleans Medical and Surgical Journal for May. The author considers the evils of tight lacing as affecting—1. The osseous system; 2. The thoracic viscera; 3. The abdominal viscera; 4. The pelvic viscera.

1. The principal parts of the osseous system are the spinal column and the ribs. The former rarely becomes affected from this cause; when, however, it becomes so, the injury is of a serious nature—caries or necrosis. The ribs are more frequently injured, the most frequent result being the deformity called "chicken-breast." This means a turning inwards of the ribs from continued pressure from without, materially diminishing the space occupied by the lungs. The angle of the ribs is thereby made more acute, and they are rendered more liable to fracture.

2. Among the thoracic viscera, the *lungs* suffer most from this abominable fashion. They are not allowed to expand to their full capacity—the arterialization of the blood is interfered with: cold feet, cold hands, headaches, nervousness, debility, dullness and weakness of mind, are the result of a deficiency of oxygen in the blood. The lungs become predisposed to tubercular deposit by tight lacing, and chronic enlargement of the air-cells, producing a state similar to what is called "heaves" in horses. The heart, of course, suffers at the same time; it is over-taxed, becomes first irregular, and later, not unfrequently the seat of organic disease.

3. The liver, stomach and intestines suffer among the abdominal organs. The liver is crowded in an unnatural position and shape, its circulation obstructed, the glycogenic function interfered with, and thus the foundation is laid to many acute and chronic disorders. The stomach suffers with the other organs in the general debility; it is prevented from developing itself normally, and its rotatory and peristaltic action is interfered with. The morbid and vitiated appetite of our fashionable tight-lacing ladies is frequently attributable to this cause. The pressure upon the intestines produces various evil results. It diminishes the peristaltic motion, obstructs the circulation, especially the portal; interferes with the proper performance of the function of the

absorbents, and, by diminishing the abdominal cavity, forces the intestines into the pelvis; in this way—

4. The bladder and the uterus are made to suffer. The capacity of the former is diminished, and either frequent micturition or retention of the urine may result, and predispose to the deposition of stone. More harm is done by pressure on the uterus than on any other organ. The whole catalogue of retro and ante-versions, retro and ante-flexions, prolapsus and procidentia uteri, find in tight lacing a predisposing as well as an exciting cause. Abortions and miscarriages, with their long train of bodily and mental suffering, are produced by pressure upon the abdomen from this miserable practice.

We have perused the paper of Dr. Young with great pleasure, and fully agree with him when he remarks, that it is the imperative duty of the physician to teach woman "how much better it is to *live* unfashionably than to *die* fashionably."

A New Book, by Dr. Alfred C. Garratt, of Boston, has been issued by Ticknor & Fields, of the same city. It is entitled, "The Medical Uses of Electricity in the Treatment of Nervous Affections." It is a thoroughly systematic work, of over 700 pages, and nearly 100 cuts, showing not only the best methods for the therapeutical employment of electricity in the various nervous affections, but also showing the anatomy of the parts to be involved, and a concise view and means of diagnosis of the great variety of nervous diseases met with every day. The work proposes to fill a vacancy which exists in medical literature upon this subject.

A New 'Pathy.—A distinguished physician of Paris, M. Marshal de Calvi, is now lecturing on a new medical doctrine, to which he has given the name of "Holopath." M. Marshal considers that diseases, as they come before the medical practitioner, are only phases or episodes of a general affection of the organism, which affection or diathesis produces the episodes when circumstances favor their appearance. The lectures are causing some sensation in the French capital.

APHORISMS ON THE HYGIÈNE AND NURSING OF INFANTS.

[Translated from the French for the Western Lancet.]

THE child should be subjected to hygiènic regulation from its cradle, in order to sustain its constitution if it is good—in order to ameliorate it if bad.

We must combat, in early infancy, the scrofulous, gouty and syphylitic dispositions inherited from the parents.

A man with impure blood should never think of perpetuating his race.

A woman who becomes *enciente* should renounce those habits, pleasures and fatigues, which may exercise an evil influence upon the health of the fœtus, if she wishes to give birth to a healthy child.

Bloodletting has a good effect upon gestation, but it should not be used unless plethora, local or general, is present.

Denial of the unreasonable caprices of a pregnant woman can not have any influence upon the health of the infant.

A woman can and ought to nurse her child, if she is in good health, and if her parents or immediate relations are not scrofulous, consumptive or cancerous.

There are women of good constitution unable, nevertheless, to nurse, for their milk is small in quantity, badly elaborated, and dries up from the slightest causes.

A woman in whom the mammary secretion is very active previous to her accouchement, is almost always a good nurse.

A mother who nurses can commence six or eight hours after the birth of the child.

A woman who nurses should not suckle the child oftener than every two hours.

An infant that takes the breast at regular intervals, sucks with more avidity than others, and drains the breast of all the milk it contains; and it is the part last obtained which is the best, as it contains more cream than the first parts of the flow.

Between eleven o'clock at night and six or seven in the morning, a good nurse need only suckle the child once.

It is dangerous to take for a hired nurse a primiparous woman; she is necessarily inexperienced.

A good nurse is from twenty to thirty-five years of age, with brown hair, the gums bright red, the form inclined to *embonpoint*, the breasts well formed, firm, and marbled with bluish veins.

A nurse should not have any mark, recent or ancient, of scrofula or syphilis.

The milk yellowish in the first month after birth, and bluish white afterwards, is an alkaline emulsion formed of water and solid principles dissolved oa suspended.

The butter is only suspended in the liquid; the other principles are dissolved.

The milk should be abundant to be profitable.

The first part of the milk drawn from the breasts is serous, the second part is thicker, and it is the last part of the draught which is the richest and the most charged with cream.

The milk (examined by the microscope) should be filled with globules, numerous, tolerably large, and well-formed—for small globules, resembling dust, are a sign of its bad elaboration, and of its insufficiency.

Too few, or too many globules, are equally injurious.

The milk varies in its composition according to the idosyncrasy, temperament, constitution, the time elapsed since the accouchement, the time since the last repast, the regimen of the nurse, the action of the genital organs, etc.; but the differences are not so great as to modify the precept—"If the infant thrives, then the milk is good."

The milk is altered in composition by the febrile state, and by acute and chronic diseases.

Fever diminishes the quantity of milk, reduces the number of its globules, and concentrates its solids in a smaller proportion of water.

The effect is the same, in different degrees, in all acute affections, and in some chronic ones.

Pus is sometimes mixed with the milk in cases of abscess of the breast.

The milk of a healthy nurse, which is too rich or too highly charged with solid elements, is indigestible, and causes diarrhœa.

Milk altered, reduced and impoverished by fever or disease, also causes diarrhœa.

The influence of diseases upon the composition of the milk is not special and specific, for they all have the same effect, which is the same as that of fever.

Milk altered in its composition by fever or disease does not always exercise an injurious influence upon the health of the child.

Whatever may be the cause of alteration in the composition of the milk, the result is always the same for the infant—the accidents which arise have always for their seat the digestive canals, and diarrhœa is always the consequence.

Milk which does not present any alteration appreciable to chemical analysis, may yet be altered in its intimate elaboration in such a manner as to make it an injurious aliment.

Spasms, or instantaneous convulsions, result ordinarily from changes caused in the secretion of milk by mental affections, too lively emotions and impressions, agreeable or painful, experienced by the nurse.

Mental impressions dry up, suddenly, the secretion of milk, or modify seriously the proportion of its solid elements.

The happiness which a woman feels in fulfilling her duties of nurse, is the cause of the internal sensation at the moment she is going to nurse the child, known as the *draught.*

The premature return of menstruation in a nurse modifies slightly the chemical composition of the milk, and injures its elaboration; but if the infant does not appear to suffer, which often happens, the nurse may be retained.

A nurse should abstain from sexual intercourse if she experiences great excitement.

A nurse should likewise abstain through fear of pregnancy, which modifies the milk in quantity and quality, so as to render it injurious to the child.

A change of nurses has no injurious effects when necessary to replace a poor one by a better.

The nurse should be changed as often as may be necessary.

Suckling, by mother or nurse, may give place to artificial feeding.

Feeding by the nursing-bottle is far inferior to suckling, although, when well carried on, it sometimes yield highly satisfactory results.

Artificial foods should be administered during the earliest periods of life, by means of the nursing bottle, filled with tepid milk diluted with barley water or oatmeal gruel; afterwards, with milk alone.

An infant needs nothing more than milk during the first months of life. At the age of six months it may commence to take light soups.

Greasy articles of food should not be given until after the first year.

The time of weaning should be fixed between the twelfth and twentieth month.

One of the periods of repose in the progress of dentition should be chosen for weaning — that which comes after the appearance of the first twelve or of the first sixteen teeth.

Weaning should be commenced by keeping the child from the breast during the night.

After some weeks' separation from the mother at night, the child should be denied the breast in the daytime also, and it thus arrives at an independent existence.

Infants and children should be carried into the sunlight and open air in all kinds of weather.

Clothes which fit the body without constriction are preferable in all weathers to loose ones, which expose different portions of the skin to the cold.

Infants should be washed in tepid water every day, and as they become habituated to it, in water nearly cold.

Dr. Livingstone and his party were at Kongone on the 12th of December last. They had all suffered with fever, but were recovering, and in good spirits.

Therapeutical Record.

Pills of Carbonate of Ammonia in Chronic Bronchitis.—(By Dr. John Williams, of Cork).—Dr. Williams recommends the following formula for administering carbonate of ammonia in pills, which, he says, are of great service in chronic bronchitis, and especially in those cases where the bronchial secretion is viscid and expectorated with difficulty:—

℞—Ammoniaci .....gr. ij;
Pulv. ipecac. .....gr. ½;
Morphiæ mur. .....gr. ⅙;
Ammoniæ carb. .....gr. ij;
Mucilaginis acaciæ....q. s.
M.—Fiat pilula.

The pills should be at once coated with a varnish of balsam of tolu, dissolved in chloroform, and they should afterwards be kept in a bottle. —*London Pharm. Journal.*

Treatment of Excessive Perspiration of the Feet.—(By M. Gaffard).—It it well known that excessive perspiration of the feet may be a very troublesome complaint. It not unfrequently produces excoriation between the toes, giving rise to an exudation of a disgustingly fetid odor; and it sometimes occasions ulcerations, which render locomotion very painful, or altogether impossible, forcing the persons affected to interrupt their business occupations. M. Gaffard, of Aurillac, recommends the following means, which he says he has employed in such cases with complete effect. The treatment consists in pouring between the toes a few drops of a liquid, composed of one gramme (fifteen grains) of red oxide of lead, and twenty-nine grammes (about an ounce) of the solution of sub-acetate of lead (of the French Pharmacopœia): the sesquioxide of lead is pounded in a mortar of porcelain till it is finely divided; the sub-acetate is added gradually, and the whole is put in a bottle, which is shaken each time it is used. This application made every eight days is sufficient, in most cases, according to M. Gaffard, to cure the affection, and prevent its return. The liquid, without completely stopping the

perspiration, moderates its amount, and regularizes the action of the morbid surfaces. The perspiration becomes inodorous, the skin regains its original thickness at the excoriated parts without losing its pliancy, and the parts return to the natural condition of cleanliness and health. —*Rép. de Pharmacie*, and *Edinb. Med. Jour.*

Cod-Liver Oil Panada.—We are informed by the Bulletin de Thérapeutique that Mr. Bassi has recently proposed, under this denomination, a new method of exhibiting cod-liver oil, which has permitted this medicine to be taken, not only without reluctance, but with pleasure.

Take eight ounces of white bread broken in pieces, toasted at a moderately hot fire, throw them into a tinned saucepan with five pounds of water; boil down to one half; strain with slight pressure, and heat gently until the fluid assumes a gelatinous consistency; add 3½ ounces of loaf sugar and 2 ounces of isinglass. Remove the mixture from the fire, and allow it to cool; add 40 grains of tartaric acid, and mix.

The bread jelly being thus prepared, take—

Bread jelly	4 oz.
Cod-liver oil	1 "
Cinnamon water	½ "
Essence of lemon	12 drops.

Mix well in a glass mortar.

Muriate of Ammonia in Nervous Cephalalgia.—(By Professor Barallier).——Professor Barallier, of Toulon, reports that within the last three years he has administered this substance in 259 cases of nervous cephalalgia, and with success in 202 of these. He gives forty-five grains, combined with mint-water and syrup of orange-peel, divided into three doses, to be taken at intervals of half an hour, amendment commencing after the first dose, and the third frequently not requiring to be taken. To prove effectual, however, the remedy should not be given at the very commencement of the paroxysm, but when it has acquired great intensity. This agent not only gives relief to the urgent pain of the paroxysms, but, after having been had recourse to on several occasions, diminishes the number and frequency of these.

To be of use, it must not be indiscriminately used for every cephalalgia; and the result of the analysis of M. Barallier's experience leads to the following conclusions: 1. The muriate almost constantly dissipates paroxsyms of idiopathic migraine, and of migraine consecutive to too abundant menstruation. 2. It is powerless in the hemicrania which is dependent upon irregularity or suppression of the menses. 3. It is tolerably successful in cranial pains dependent upon disorder of the stomach, and in the accidental cephalalgia frequent in women and feeble persons under the influence of sudden changes of the atmosphere, prolonged intellectual labor, or moral emotion. It operates beneficially in cephalalgias consecutive to repeated paroxysms of intermittent fever; those which are observed during the decline of severe fever, and in the course of the irritative period of typhus.

—*Bull. de Thérap.*, t. lvi., and *Med. Times and Gaz.*

Tincture of Capsicum for Chilblains and Toothache.—(By Dr. A. Turnbull).—My plan of treatment is simply to saturate a piece of sponge or flannel with the concentrated tincture of capsicum, and to rub well over the seat of the chilblains, until such times as a strong tingling and electrical (?) feeling is produced.

This medicine possesses an extraordinary power in removing congestion, by its action upon the nerves and circulation.

This application ought to be continued daily until the disease is removed. Relief will be experienced on the very first application, and frequently there will be a total removal of the disease after the second or third. This, of course, depends upon the severity of the case. This embrocation, when rubbed, never produces excoriation, if the skin is not broken.

The manner of using it for toothache is by putting a drop or two of the tincture on cotton, and applying it to the part affected; the relief will be instantaneous. The following is the formula:

℞—Tinctura capsici concentrata:
Capsici baccarum ..... ℥ iv;
Spiritus vini rect..... ℥ xij.
Macera per dies septem et cola.

It may also be made with advantage by displacement.

—*Medical Gazette.*

On the Local Employment of Chloroform in the Reduction of Dislocations.—M. Orliac, a French provincial practitioner, relates two cases of recent dislocation of the shoulder, in which rapid and painless reduction was accomplished. This result he attributes to having surrounded the shoulder with, and placed in the axilla, compressess imbibed with ten or twelve grammes of chloroform, these being applied two or three minutes prior to and during the attempt at reduction. In this way, he observes, assistants may be dispensed with (an important matter in country practice), and pain be prevented, without any danger being incurred.—*Moniteur des Sci. Méd.*, and *Atlanta Med. and Surg. Jour.*

Iodate of Potassium in Affections of the Mouth.—Induced by the great success that has attended the chlorate of potassium in affections of the mouth, MM. Demarquay have tried the efficacy of the iodate of potassium in numerous cases of diphtheritic and gangrenous stomatitis. The success has been considerable, and that in which the chlorate has failed. The dose employed was from four to eight grains.

—*Moniteur des Hóp.*, and *Georgia Med. and Surg. Encyc.*

Relief of the Tenesmus of Dysentery.—Ansaloni, in his inaugural dissertation, reports the very favorable results obtained by Dr. Leclerc, of Tours, by the combined employment of belladonna, or stramonium and calomel, in dysentery. A large plaster of extract of belladonna, or stramonii (℥ iss.), is placed on the regio pubis, and every morning and evening, for a few days, a grain and a half of calomel administered. The belladonna and stramonium may be alternated. Tenesmus soon yields to this treatment, as well as all other symptoms of dysentery.

—*Phil. Med. and Surg. Reporter.*

Diphtheria.—A writer, in the Lancet and Observer, gives the following in diphtheria:

℞—Chlorat potas. pulv..... ʒ ij;
Hydrochlorid acid....*f* ʒ j;
Aqua.....*f* ℥ viij.
M.—Dose, half an ounce every two or three hours.

Reduction of Strangulated Hernia.—In the Maryland and Virginia Medical Journal, Dr. Stockdell reports a case of strangulated congenital hernia in a child three months old, that was reduced without an operation, in the treatment of which there are some points of interest worthy of making a note of. *Taxis* was tried "under the full influence of an anodyne enema, followed by a warm bath"; also, subsequently, "under the influence of an *anæsthetic*," but entirely without favorable results. "A tobacco leaf was procured, and, thoroughly moistened with warm water, applied to the whole abdominal parietes. At one o'clock, its effects being only slightly perceptible in the pulse—the abdominal distension remaining unchanged,—it was removed, and flannel cloths, wrung out of a very strong, hot infusion of tobacco, directed to be applied in its stead, and frequently changed." The tobacco was continued from nine in the morning until seven in the evening, when the hernia was spontaneously reduced.

Removal of Stains of Nitrate of Silver.—(By T. Collins). —If a solution of cyanide of potassium be added to a solution of nitrate of silver, a whitish precipitate of cyanide of silver immediately occurs. This precipitate is perfectly soluble in an excess of the cyanide of potassium, forming a colorless solution.

Black stains produced by nitrate of silver on the skin, nails, teeth, and on linen, &c., brushed over with a solution of cyanide of potassium (eight or ten grains of the salt to ℥j of distilled water) are similarly acted on. They will disappear after one or two applications, if at all superficial; if even deeply seated in the textures, they may require several.

The cyanide of potassium is a cheap salt, and may be procured from any respectable druggist. The solution should be made fresh, as it is liable to spontaneous decomposition.

—*Dub. Med. Press.*

Dyspepsia.—The Louisville News gives the following for dyspeptic cases:

℞—Tinc. cinchon..... } *aa f* ℥ij;
" quassiæ ..... }
" nux vomicæ.... *f* ℥j.

M.—A teaspoonful three times daily, in a wineglass of sweetened water.

Uva Ursi.—Mr. Cyrus Hendricks, Jr., of Litchfield Corners, Me., writes as follows: — "I have employed uva ursi six times in lingering labor and abortion, with good effect. I used one ounce of uva ursi to one pint of boiling water, giving half an ounce every fifteen minutes."

Menorrhagia.—Dr. Burns recommends, in the Belmont Medical Journal, the following prescription for this affection:

℞—Tinc. cinchona comp., ʒ iij *f*;
Tinc. cantharides .... ʒ ij *f*.
M.—Dose, a teaspoonful three times daily.

Chloroform.—M. Orliac, in the Moniteur des Hôpitaux, relates two cases of recent dislocation of the shoulder, in which painless reduction was accomplished by surrounding the part with compresses saturated with chloroform.

Glycerine.—Simon, of Berlin, uses an ointment composed of five parts of glycerine, and one part of starch, and says that it forms a smooth butter-like substance, free of all smell, exciting no chemical action, and unaffected by temperature.

Gelseminum.—Dr. Pattee states, in the Journal of Materia Medica, that nervous headache, neuralgia and coryza can be readily cured with this drug.

Indian Corn.—Dr. Gambling, in the Nashville Journal, has an article upon the use of Indian corn as a periodic. He recommends corn meal as a substitute for quinine, and states that he has used it in thirty cases of intermittents with favorable results.

Atropia.—Dr. Pattee, of Massachusetts, advises this substance in "incontinence of urine." The following is his prescription:

℞—Atropia.....gr. i;
Aqua distil. ..... ℥ v;
Acetic acid.....gtt.vi.
M.—Dose, one drachm three times daily. For children, the dose must be properly graduated.

Chlorate of Potash in Mercurial Salivation.—Dr. Caldcleugh, of Arkansas, reports, in the Nashville Journal, several cases of decided mercurial ptyalism, in which five-grain doses, repeated, of the chlorate of potash, dissolved in an ounce of water, acted promptly in relieving the symptoms. We have, in scores of cases, tested the efficiency of this same remedy under like circumstances. It is, beyond all comparison, the most prompt and beneficial remedy we have ever tried for the relief of mercurial salivation.

Dysentery.—Dr. F. G. Smith, in the Medical and Surgical Reporter, advises the use of this prescription in dysentery:

℞—Saturated solution sulp. magnesia, *f* oz. vij;
Aromatic sulphuric acid ..... *f* oz. j.

M.—Dose, for an adult, one tablespoonful in three ounces of water, every four or six hours. Morphia may be added to each dose, if there is much pain.

Oxalate of Cerium.—Dr. Simpson recommends this salt as an infallible cure for the vomiting incidental to pregnancy. He administers one or two-grain doses, three times daily.

Dr. Awenarius, of St. Petersburg, first employed this volatile alkali in the treatment of cases of rheumatism, and treated, during two years, more than 250 cases of rheumatic affections successfully with it. He affirms that the pain and fever disappeared the day after its administration. His formula is:

℞—Propylamine.....gtt. xx.
Aquæ distil. ..... *f.* ℥ vj.

Add, if necessary, essence of peppermint, with some sugar.

Podophyllin.—Professor Kirtland proposes the use of this article in combination with Leptandrin, as a substitute for calomel. He gives:

℞—Podophyllin .....
Leptandrin..... } *aa* x grs.

M.—Divide into forty powders. One powder at bedtime, and repeat when necessary.

MEDICAL COLLEGE OF VIRGINIA,

AT RICHMOND:

SESSION OF 1860-'61.

THE ANNUAL COURSE OF LECTURES will commence on the first Monday in October, and continue until the 1st of March.

CHAS. BELL GIBSON, M.D.,
PROFESSOR OF SURGERY AND SURGICAL ANATOMY.

DAVID H. TUCKER, M.D.,
PROFESSOR OF THEORY AND PRACTICE OF MEDICINE.

LEVIN S. JOYNES, M.D.,
PROFESSOR OF INSTITUTES OF MEDICINE AND MEDICAL JURISPRUDENCE.

BEVERLEY R. WELLFORD, M.D.,
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JAMES B. McCAW, M.D.,
PROFESSOR OF CHEMISTRY AND PHARMACY.

MARION HOWARD, M.D.,
DEMONSTRATOR OF ANATOMY.

The recent appropriation of thirty thousand dollars to this Institution, by the Legislature of Virginia, will enable the Faculty to make large additions to the means of instruction heretofore placed at their command. The illustrations in every department will be greatly multiplied and improved; and a commodious Hospital is now in course of erection, in immediate proximity to the College, which will materially extend the facilities for Clinical instruction. Clinical Lectures will be delivered twice a week by the Professors of Surgery and Practice of Medicine, and operations performed before the class as occasion may require.

Students also have access to the wards of the Richmond Almshouse.

The supply of material for dissection is ample, and the winter climate of Richmond highly favorable to anatomical pursuits.

Instead of the single "Warren Prize" of $100, which has been offered to the graduating class at the last two Sessions, *two prizes of* $50 *each* will be offered,—one for the best Essay on any Surgical subject, the other for the best Essay pertaining to the Theory or Practice of Medicine.

FEES:

Matriculation	$ 5	Dissecting Ticket	$ 10
Professor's Ticket (each $15)	105	Graduation	$ 25

☞ For further information, address L. S. JOYNES, M.D.,
Dean of the Faculty.

NORTH CAROLINA
MEDICAL JOURNAL.

AUGUST, 1860.

Original Communications and Translations.

ON THE USE OF TOBACCO, AND ITS INJURIOUS EFFECTS UPON THE HUMAN SYSTEM.

BY J. T. SHAFFNER, M. D., OF SALEM, NORTH CAROLINA.

(A Prize Essay, for which Fifty Dollars was awarded by the N. C. Medical Society.)

QUÆ PROSUNT OMNIBUS.

We live in an age remarkable for its vast discoveries, for its wonderful developments in knowledge, the sciences and the arts. Among the sciences, in which such wonderful improvements have been made, that of Medicine claims high rank,—a science, than which none other is more rapidly advancing. Its progress has been, and still is, onward. Important facts are rapidly being made known, and new views, throwing light on subjects heretofore considered doubtful and obscure, have been proposed, and are daily becoming more numerous and comprehensive.

Medicine is neither a finished nor a perfect science. It is not finished, because it still admits of an increase of light, certainty and power. It is not perfect, otherwise no doubts would be entertained of the nature and treatment of any disease which the physician is called upon to cure. It owes much to experience—an experience at once discriminative and profound. The collected light of ages beams around its path. To the assiduous and enlightened cultivators of the art, from the days of Hippocrates to the present age of the world, we are indebted for that fund of facts, and rational deductions, which go to constitute the science of curing diseases.

No one individual, in this age, should arrogate to himself an original experience, completely independent of the past experience of the medical world. Indeed, the most skillful in the art pretend to nothing more than a judicious combination of the experience of the past with their own observations; an application of that knowledge derived from erudition, with that acquired by personal inspection; a careful analysis of the opinions of their predecessors, so as to build up a manly and self-sustained judgment, which shall enable them to disentangle the perplexities of disease, and send a discriminating eye over all those details of medical practice which are so apt to confuse and embarrass the mind. It would, therefore, be folly to presume entire originality for the following Essay; hence, I freely confess, that all things presented are the result of research, acquired and collected by toil, from an accumulated experience of centuries. Relative facts lie scattered over such a vast field of inquiry, and are of such difficult investigation, that none but a mind disciplined by previous habits of study can surmount the obstacles which hinder a proper deduction of the most valuable and important portions.

But, partaking of the spirit of the age in which we live, and prompted to this effort by a zeal for improvement, with just ambition for some distinction, I have taken the boldness to enter, as competitor, the number of those who will present to the "Medical Society of the State of North Carolina," Essays on this subject; and whilst, with a spirit of the true interpretation of nature, I assiduously employ the powers of my own mind to discover and present whatever is important, I shall not neglect to advert to other minds, in order to derive from them that light which my experience alone can never confer.

My design, in the following observations, is, in the first place, to refer to the natural history of Tobacco, and to treat of it as an organic substance; in the second, to consider its various uses; in the third, to institute an inquiry into the mode by which it acts upon the system; and, finally, to particularize the most common forms of disease engendered by it.

As to the history, we have various and conflicting accounts. It is believed to have originated in Central America. Columbus

found the Cubans using it in 1492, and Cortez found it in use among the Mexicans. It is, again, said to have been discovered by the Spaniards about the year 1560, in a small island called Tobasco, in the bay of Panama, in the South Seas, whence the name. Others, with more probability, contend it was found by the Spaniards in a province of Yucatan, New Spain, called "Tabaca," and thus derived its name. However this may be, we have abundant evidence that it is a native of tropical America, where it was known and used by the aborigines for centuries. Its botanical name, *Nicotiana*, was given in honor of Mr. John Nicot, who, being agent in Portugal for Francis II., king of France, came to the knowledge of it by means of a Flemish merchant, and on his return from Lisbon presented it to the queen, wherefore it has also been called *Herba Regina*. This claim is, however, disputed by others, and the truth will probably remain forever in mystery. It was first taken to England, by Sir Walter Raleigh, in the year 1586, and by him presented to the "virgin queen," Elizabeth.

The plant is of annual growth, and possesses a large fibrous root, which is divided into many parts; it has a stout, erect, hairy stem, and generally attains a height of five or six feet, depending upon season and soil. The flowers are large and beautifully red, growing on the tops of branches, and on the main stalk. The capsule, which succeeds the flower, is bi-valved, large, and of an oval form, opening longitudinally, shedding very minute, though abundant seeds.

We have accounts, by different authors, of several species of this plant, and botanists generally reckon *seven*. I shall confine myself to the principal and officinal one, viz., *Nicotina Major Latifolia*, or the large broad-leafed Tobacco. This is the most valuable of the species, is cultivated in all temperate climates, but is very liable to be injured by premature frost. Nowhere does it appear to flourish better than in the United States, where it is most extensively cultivated, and, in some portions, to the entire exclusion of other products.

As found in commerce, it presents a yellowish-green color, has a peculiar nauseating, oppressive odor, and a bitter, acrid taste.

In the United States Pharmacopœia we have the leaves alone the officinal portion, but all parts possess medicinal properties. All of these are imparted to water and alcohol; long boiling, however, destroys them, hence the extract is not so efficacious as the infusion. Many chemists have subjected tobacco to analysis, the result going to prove it consists mainly of two active principles—one, an acrid volatile substance, with little color, in which alkaline properties have been recognized; the other, a camphoracious, volatile oily substance, found in much less quantities, but by far more powerful in action. To the former has been given the name of *Nicotin.* It was discovered and first described by Vanquelin, but others subsequently obtained a purer article. It is a nearly colorless liquid, in specific gravity heavier than water, very irritant to the nostrils when inhaled, and said to be very poisonous. The latter was first described by Hermstadt, and called *Nicotianin.* It is believed to be the odorous principle of tobacco. We have it generally described as colorless, but it is more generally of a yellow color, becoming darker by exposure. It has a pungent, irritative odor, an acrid burning taste, and is most virulently poisonous. Giddiness and nausea are induced when taken in minute doses, and in larger, all the symptoms of acrid poisoning are developed. Besides those two principle constituents, an empyreumatic oil has been obtained by distillation, which, though at first colorless and limpid, becomes, by long standing (and as it is generally found in the shops), a dark brown, viscid substance. Its properties are similar to the preceding, being equally acrid and poisonous. In the United States Pharmacopœia it is officinal as *Ol. Tabaci.*

The uses of tobacco are various, both medically and as a luxury. Medically, its properties may be divided into those that are real and those that are *supposed.* The real were generally known among physicians even at an early period, who ascribed many virtues to it, in obstinate diseases. Certainly some uncommon benefits ought to be derived from it, otherwise its use could not, we would suppose, be continued in defiance of every sentiment of cleanliness and delicacy. It may, from the experiments and corroborating assertions of many authors, be pronounced a valu-

able acquisition to the Materia Medica, and more extended inquiries, relative to its properties, might very appropriately be made. It is said to be stimulant, sedative, narcotic, sudorific, diuretic, emetic, cathartic, anthelmintic and errhine.

Farther reference is here uncalled for. The supposed medical properties are numerous. Among the ancients, those herbs most fetid in smell and bitter in taste were considered most powerful and valuable as medicines. The poet Virgil makes mention of a countrywoman pounding garlic, and some similar herbs, which he terms *herbes alenses* ("ill-scented herbs"), for the comfort and support of the reapers, whose strength had become exhausted by intense labor and heat. Probably this was done by way of charm, for we know full well such properties are not inherent of garlic, &c. In like manner divine virtues and miraculous powers have been attributed to this highly-favored weed by many, even at the present day, who are inclined to regard it as a great panacea; but these will generally be found habitual and immoderate consumers, either with the pipe, the snuff-box or quid, hence, naturally enough, endeavor to justify and make the best of a habit highly disgusting and pernicious. This leads us to consider its uses habitually.

The vegetable kingdom has probably never produced an article, the use of which has been so rapidly and universally extended. Statistics show an enormous consumption of the article, which, notwithstanding its deleterious consequences, so well known to all, is gradually on the increase. The consumption in this country is immense. In Europe, from the plains of the Castile to the frozen Archangel, the pipe and the cigar are a common solace, among all ranks and conditions. In vain was the use of it prohibited in Russia, and the knout threatened for the first offence, and death for the second! In vain Pope Urban VIII. thundered out his bull against it! In vain James I. wrote his "Counterblaste" against it! In vain the Puritan Fathers of Massachusetts endeavored to abolish its use, some wishing to hang for every offense, considering all violations of the word of Jehovah sins only to be cured by the simple and uniform process of cutting off the transgressor. Opposition only excited more

general attention to the plant, awakened curiosity regarding it, and promoted its consumption. So, in the Far East: the priests and the sultans of Turkey and Persia declared smoking a sin against their holy religion; yet the Turks and the Persians became the greatest smokers in the world. In Turkey the pipe is perpetually in the mouth. In India all classes and both sexes smoke; in China the practice is so universal, that every "female, from the age of eight or nine years, wears, as an appendage to her dress, a small silken packet, to hold tobacco and a pipe."

It is even argued by Pallas, that the extensive prevalence of the practice in Asia, and especially in China, proves the use of tobacco for smoking to be more ancient than the discovery of the new world.

"Amongst the Chinese," he says, "and amongst the Mongol tribes, who had the most intercourse with them, the custom of smoking is so general, so frequent, and has become so indispensable a luxury,—the tobacco purse, affixed to their belt, so necessary an article of dress,—the form of the pipes, from which the Dutch seem to have taken the model of theirs, so original,—and, lastly, the preparation of the yellow leaves, which are merely rubbed to pieces, and then put into the pipe, so peculiar, that they could not possibly derive all this from America, by way of Europe, especially as India, where the practice of smoking is not so general, intervenes."

I scarcely violate the editorial sanctity of Harper's Magazine, when I refer to the number of that world-read periodical for June, 1855, containing an opening article, entitled, "The History and Mystery of Tobacco," with a round dozen of characteristic illustrations. It covers nearly twenty pages in the Magazine, and contains the concentrated essence of two hundred. In that article is combined a better history of tobacco than has yet been written, and the illustrations are worthy of it. First, in a free-and-easy group, we see "*the Smokers of all Nations,*" of that

> "Sublime tobacco! which, from east to west,
> Cheers the tar's labors, or the Turkman's rest,—
> Which on the Moslem's ottoman divides
> His hours, and rivals opium and his brides,—
> Magnificent in Stamboul, but less grand,
> Though not less loved, in Wapping or the Strand."

Then comes the "Great Spirit," smoking on a magnificent scale, his hallowed spear applied to nothing less than a volcano, which he uses as a tobacco pipe! Next, Reveries of a Cigar, with pleasant pictures in the smoke-cloud which the young puffer was emitting. There, too, the Hindoo, with the amber-tipped "hookah" between the lips;—Mexican loveliness, playing on a balcony, with the delicate cigarette;—the rising generation, prematurely old, and unmistakably loaferish, educating themselves into a liking for the plant;—the bond of sympathy, exhibited by one stranger in the street giving light to another from his own cigar;—the comfort of smoking, as shown by an Hibernian apple-woman, with her stall under a lamp-post, indulging in the use of that short clay-pipe, classically called "*a dudheen!*" The illustrations are admirably worthy of the text, which is in the happiest vein, crowded with facts, and sparkling with quiet humor.

The largest producers, as well as the greatest consumers of tobacco, are the people of the United States, the crop of 1850, according to the last census, amounting to 200,000,000 pounds, an estimate rather beneath than above the quantity! Over 28,000,000 pounds of it were consumed in Great Britain in 1851, and I would judge, from its much greater use in this country, that for every pound used in the old, ten must be consumed here. The rapidity with which its growth and consumption have increased, in almost every country, may be inferred from the following data:—In 1662, the quantity raised in Virginia, the chief producer of tobacco on the American shores of the Atlantic, was only 60,000 pounds, and the quantity exported from that colony in 1689 had already reached 120,000 pounds. Thus, while in 1689 the quantity exported to Great Britain was only 20,000 pounds, it now averages from 30 to 40,000 pounds annually. To this may be added the contraband, as the heavy duty imposed is a good temptation to smugglers.

With us tobacco is principally used in smoking and chewing, though snuffing is not neglected; in England, it is as much snuffed as smoked: chewing is rare, being chiefly confined to sailors.

The question is often asked, "*Is the use of Tobacco injuri-*

ous?" Viewing the question in the abstract, I should answer most decidedly, *Yes!* To a person in full health, nothing is required but pure air, food and drink; everything else is superfluous, and consequently oppressive to the constitution. A narcotic substance must be more than oppressive, because it makes a direct attack upon the nervous system. It affects the stomach and the brain. Whilst drinking alcohol is a great and positive evil, using tobacco is none the better. Both should be banished, as decidedly inimical to health and reason. Whilst we come to this conclusion, however, we should not be too hasty to censure, and deprive the tobacco consumer of his consolation, but would rather keep the practice from excess. Nearly all of us habitually violate the laws upon which the well-being of the body depends, either by indulging our appetites at the expense of our stomachs, overtaxing our physical or mental powers, foolhardy exposure, neglect of obvious safeguards against dangers, or some other positive or negative contravention of the conditions of health and safety. And these things we do deliberately, and with a full knowledge of the deplorable consequences. Hence the fact, that hundreds of thousands of well-informed persons smoke, chew and snuff tobacco (universally regarded as injurious), in reckless defiance of all the evils it is calculated to entail, is but one example of the manner in which we violate the laws of health and abuse our reason.

The other day I saw a group of boys, the youngest scarcely six years old, puffing cigars at a great rate. A gentleman passing by, with a lady to his arm, reproved them severely, upon which they slunk away, hanging down their heads, apparently much ashamed of themselves: and yet their mentor, at the very moment he was administering reproof, gave unmistakable signs that he was chewing tobacco: his mouth was full of the same weed which he berated them for smoking. Verily, it was Satan rebuking sin!

I will now separately and briefly consider the habits of smoking, chewing and snuffing.

Smoking has become almost universal, and is pursued more as an amusement, or an assistant to time-killing, than for

any good expected to be derived from it. Of all the uses of tobacco, this is probably the most ancient. We read in the earliest works on the American aborigines, of the "pipe of peace," which was always employed by contending tribes, when about to treat for friendship. The habit has been constantly on the increase. At the present day it is almost universal: *man smokes; even woman smokes.*

A passion for inhaling the vapor of tobacco, through the clay of "Silicia," and the amber of "Konigsberg" and "Mernel," seems to pervade the male world, and especially of America. The mania "runs like the cholera," and is frightfully intense. Young America talks "meerschaum," reeks meerschaum, and the capillary tubes of his adolescent moustache are surcharged with meerschaum. His clothes, his breath, his pores, exhale narcotine. To color a bit of porous clay, is one of the grand aims of his life; to die before it blackens, is to leave the world with his destiny only half accomplished. When his silicious tube is new and pure, he is ashamed of it: he pulls away at it alone and in secret: he smokes in it strong nerve-shattering cigars, provocative of a thirst for "cock-tails," in order to impregnate it the sooner with the essential oil of the weed, and when it has become an abomination to the sense of sight and smell,—a thing from which even a tobacco-worm would turn with disgust, he is proud of it, exhibits it triumphantly to his smoking acquaintances, and parades it ostentatiously in the fashionable thoroughfares.

The married "*meerschaumite*" is, as a general thing, incurable. He stands upon his privileges, as the head of his household, and puffs the noisome fumes from his volcano into his wife's ringlets with the most provoking *sang froid.* Its stale vapor clings to her lace curtains, gets into her bureau, and contaminates her wardrobe. It insinuates itself through the keyhole of her *boudoir*, finds its way into her piano, penetrates her writing-case, and spoils the flavor of her perfumed note-paper. No fragrant essences, no *sachets* filled with odoriferous gums and spices, will avail against the meerschaum—it overpowers and extinguishes them. All the poor wife can do, is to keep as far as possible from her volcanic spouse, while in a state of eruption,

and refuse to kiss him afterwards. If he loves her better than four inches of smoke-polluted clay, and the poisonous, stupefying vapor he draws through it, perhaps he may give it up, and become a kissable personage. But, alas! when a man marries, he seems to think he has acquired an indefeasible right to smoke, and that Hymen's torch was made expressly to light cigars!

Bachelor *meerschaumites*, if in love, may be reformed. See to it, young ladies, that you consent to marry them conditionally, promising to forego the use of tobacco in any form. Say to them, "Choose, sir, between me and meerschaum,—you can not have both!" If you do this, you may effect much.

The following anecdote is told of the great Napoleon: "Napoleon once took a fancy to smoke, for the purpose of trying a very fine Oriental pipe presented to him by a Turkish or Persian embassador. Preparation having been made,—the fire having been applied to the recipient, nothing more was to be done than to communicate it to the tobacco, but that could never be effected in the way taken by his majesty for that purpose. He contented himself with opening and shutting his mouth alternately, without the least in the world drawing in his breath. 'How the devil!' cried he, at last,—'that does nothing!' I made him observe that he made the attempt badly, and showed him the proper mode of doing it. But the emperor always returned to his kind of yawning. Wearied by his vain attempts, he at last desired me to light the pipe. I obeyed, and returned it to him in order; but scarcely had he drawn in a mouthful, when the smoke, which he knew not how to expel from his mouth, turned back into his palate, penetrating his throat, coming out by the nose, and blinding him. As soon as he recovered breath, he exclaimed, 'Take that away from me!—what abomination! Oh, the swine!—my stomach turns!' In fact, he felt himself so annoyed, at least for an hour, that he renounced forever the pleasure of a habit which, he said, was only fit to amuse sluggards."

Smokers are sometimes summarily dealt with in Russia. Smoking in the street is strictly prohibited. The emperor, while walking one day, met a Frenchman, smoking a cigar. He approached the latter, and asked if he was not aware that it was

contrary to the law to smoke in the streets. The Gaul, not knowing by whom he was addressed, replied, "that he had been in the habit of smoking in the streets of Paris, and he did not know why he should not do so in the streets of any other city." The emperor, who detests a Frenchman, left him, and proceeding to the *Coduca*, or station of a policeman, near by, gave directions as to the disposition of the smoker. The latter was immediately placed, by force, into a small wagon, without springs of any kind, and bounced over a thousand miles of bad road, to the Turkish frontier, where he was dismissed, with permission to follow the Parisian fashion.

By the stimulating effects of the smoke on the mucous follicles and salivary glands, their secretions are abnormally increased in quantity, causing free and copious expectoration, and since the saliva is indispensably requisite to digestion, it seems rational to suppose its abstraction can not but prove the direct instrument of disease; hence its influence in inducing *dyspepsia*. As already intimated, tobacco evidently possesses sedative properties, by the influence of which the tone of the stomach becomes so reduced as to cause an improper performance of its functions. I am well aware that the smoking of a pipe, or a cigar, in the morning, is extolled by many, and even physicians, for, say they, it removes all disagreeable humors from the stomach; but let it be remembered, ere this theory be adopted, that nothing can be taken from the stomach but by emesis; hence the substance which is expectorated by those who have acquired the practice is undoubtedly the mucus secreted by the tonsils and mucous follicles of the throat, whose functions are to lubricate and protect the pharynx and æsophagus. The argument is, therefore, worthless. Others allege that smoking has proved advantageous in dispersing contagion of nearly all kinds; but such valid and substantial arguments have been brought to bear against this theory, that I am not inclined to believe it possesses such properties.

"Its narcotic fumes," says a writer, "will stupefy the brain, and deaden the invigorative power of the whole nervous system. The propensities of persons to the use of this noxious weed, being once habituated to the impression of its stimulus, is equally

prevalent with that of dram-drinkers to spirituous liquors, without which, however pernicious, they become languid and debilitated, and are affected with nervous tumors."

This language is concise and to the point, and, though uttered many years ago, is still applicable, and to be respected. I am of opinion that smoking hardens the nerves, which excite those agreeable sensations, dependent upon a delicate sense of taste. It also brings on a habit of drinking, by exciting unnecessary thirst, which is often attended with many inconveniences. The Rev. Dr. Hawes, of Hartford, Connecticut, recently preached a strong sermon against the use of tobacco. He exhibited facts and statistics, showing its destruction of health and sanity, its demoralizing influence, and its useless expense. He also claimed as a fact, that its use has a tendency to create an appetite for strong drink. "The two vices," he says, "are twin brothers, the sons of the same mother,—a vitiated, perverted appetite." He quoted authorities for this, and related certain cases in Hartford; one, of a man, who declared, after a thorough trial, that it was useless to attempt to cure the desire for rum while a man continued the use of tobacco,—it caused a thirst for liquor.

A long habit not only discolors the teeth, rendering them black, filthy and disgusting, but is attended with a greater evil, viz., it causes the gums to lose the due amount of nutrition, and thus, by becoming lax and flabby, to recede from the teeth, so that they appear long and misshapen, and at length, becoming loose, fall from their sockets.

An instance is related of a clergyman, who had acquired the habit of smoking, and was scarcely ever found without a pipe in his mouth, except when performing church duties. The quantity of saliva he discharged was incredible; his teeth became loose, and of a loathsome hue, and before the age of sixty he lost all, which, preventing him from articulating in a distinct manner, reduced him to the necessity of discontinuing his pastoral duties. He acknowledged it was entirely owing to his excessive use of tobacco, which he had not resolution enough to abandon.

Chewing is a greater vice than smoking. All that has been said against smoking is applicable to chewing. A constant habit

greatly irritates the salivary glands, occasioning a great waste of their secretions. Notwithstanding the utmost caution of chewers, some saliva, tinctured by the expressed juice, will enter the stomach, which destroys the digestive powers of that viscus; hence persons habituated to this nauseous custom have their appetites, in the course of time, greatly impaired, and lose all inclination for food, which is thus supplied by tobacco. To this we may well attribute its influence in producing emaciation and decay; for, since longevity and health depend mainly upon a sound state of the stomach and a healthy appetite, the habits of those addicted to chewing become pernicious, and life is necessarily shortened.

Chewing is probably most prevalent among the poorer classes, of whom I have noticed some, long habituated, with distracted faces, the muscles drawn to that side in which the quid is generally retained.

The following communication, referring to this nasty habit, appeared recently in an evening paper, and is particularly interesting, in that the rebuke has been given by one who is himself a chewer:—

"*Messrs. Editors:* I wish to ask you to correct an evil becoming daily more prevalent. It is the habit *gentlemen* have of chewing tobacco at places of public amusement; for this offence no excuse is acceptable. There is scarcely a moment in the day that I am without a piece of tobacco in my mouth, and, frequent as are my visits to the theatre, I have yet for the first time to use that article in the theatre.

"Last evening, I accompanied a lady to the "Walnut"; a *gentleman*, sitting to the right of my companion, was chewing and spitting incessantly, from the moment we took our seats to the close of the performance. The consequence was that the lady's dress was ruined. During the evening her muff, unseen by her, fell upon the floor, into a puddle of tobacco juice. I presume it remained there for several minutes. However, when she picked it up, it was scarcely fit for a decent person to touch; whether or not it is fit for future use, you, perhaps, can tell better

than I. This is an evil demanding a drastic remedy, and it is in your power, Messrs. Editors, to apply it. Will you do it?

* * * * * * * "HEXAGON."

There can be but one reply, and that is, that a vile habit is as bad in one place as in another; and one that acknowledges that there is scarcely a moment of the day that he is without a piece of tobacco in his mouth, has little right to complain of a man who perhaps only chews when he goes to the theatre. The chewer has a right to choose his time and place for chewing. "Hexagon" prefers to make himself disagreeable everywhere except at the theatre, while ——— may give that place a preference over all others. It is difficult to say which one is most censurable.

Tobacco, when reduced to a powder, is called snuff, in which form much is consumed. The finer it is pulverized, the more powerful are its effects; hence that which is called "Scotch snuff," generally considered the best, owes its preference to the minuteness of the particles.

This habit, like the other, has but little to recommend it. I have heard some urge as a reason for its use, that it relieves or even prevents headache, and also possesses the property of clearing the sight. As a sternutatory, used medicinally, I will not deny its merits, but in the Materia Medica we have other errhines, equally trustworthy, and more innocent. How far its effects may extend cannot exactly be determined, but probably it operates, more or less, on the whole encephalon. The Schneiderian membrane is so intimately connected with the minute nerves and blood-vessels of the brain, that so finely a pulverized substance, abundantly and constantly applied to it, can not but exert some influence on continuous and contiguous tissues.

The theory is well substantiated by facts, and moreover stands to reason, that the constant employment of so powerful an errhine will draw and remove all their secretions, leaving behind the thick, which gradually accumulate, and induce, by their presence, a predisposition to apoplexy.

A writer thus expresses himself on this subject: "I am confident more people have died of apoplexy in one year, since the

use of snuff, than have died of that disease in a hundred before the use thereof; and most, if not all, whom I have observed of late to die of that disease, were extreme and constant snuff-takers!"

This extravagant language is probably near the truth; and though I am not willing to attribute, to the same extent, its agency in producing the now numerous cases of apoplexy, there can be no question that deaths have, by that disease, been gradually increasing, especially among the indolent and luxurious.

"Common snuff," says another writer, "in habitual snuff-takers, has been found penetrating into the sinuses communicating with the nose, where it has formed horrid abscesses; it is often carried into the stomach, and by the use of it the skin is tinged of a pale-brown color. The evils liable to be produced by it are—dry up the brain, emaciate the body, enfeeble the memory, destroy, if not entirely, yet in a great measure, the delicate sense of smell, and ruins the complexion!"

These results are sufficiently evident in inveterate snuff-takers. The most delicate females have their complexion entirely ruined by it. It is strange, surpassing strange, that the snuff-box shall be deemed so great a sacrifice, since for a beautiful complexion most people are willing to sacrifice every thing beside!

The use of tobacco, either by smoking, chewing or snuffing, if moderately indulged in, will not prove of decided injury. It is an established fact, that the system possesses the power of tolerating even poisonous agents, in certain quantities, without having its functions materially deranged. Thus, it is well known that alcoholic stimulants, of all kinds, if taken in excess, become highly injurious; yet it would be folly to argue, that a moderate quantity, if pure, is liable to impair health. On the contrary, deranged health is frequently restored by a judicious administration of alcohol.

Let me take another view. Nothing, perhaps, will prevent a proper performance of the functions sooner than the want of "Nature's benign restorer;" still we are well aware, from observation, that by habit, persons may accustom themselves to comparatively very little repose, and yet enjoy perfect health. So

with the moderate use of tobacco. The great difficulty is, to draw a line between its *use* and *abuse!* Like alcohol, it is so exclusive in its demands, that it reduces its victim to a complete slave. A little will, by-and-by, be more and more, until, like a terrible tyrant, it will rule with a rod of iron! When it has reached this point; when, inveterately taken day by day, it be-becomes so highly detrimental, that those who thus habituate themselves to its abuse generally, sooner or later experience its deleterious powers, in the many disagreeable emotions excited by its influence over the system; then it becomes a vice, and as such alone will I yet treat of it.

Many persons have I known, scarcely able to procure the necessaries of life, who by some means make out to procure their daily supply of tobacco. I have known whole families, and those poor too, to consume enough daily, by the value of which they might have lived comfortably. A large proportion of the daily earnings of the poor is spent in this way.

The Rev. Dr. Hawes, of Hartford, Conn., previously referred to, in his strong and trenchant onslaught upon the vice of tobacco smoking and chewing, presented the economical aspect of the habit in a forcible manner. He supposed a person to spend only six cents a day for cigars or tobacco — a very moderate estimate, — and proved that, in the course of forty years, with interest and compound interest, the sum would have nearly reached $4,000, a sum, as he well remarked, that would be found very useful to a family at a time when the faculties of the husband and father begin to fail. Again, New York city expends more than $300,000 a year for cigars only — or $10,000 a day for *cigars*, to $8,500 for bread! These statistics will apply to every community.

In the United States, *forty millions* of dollars are annually spent for this narcotic, — for the injury of the public health! The sum far exceeds that expended for all the purposes of education! What would not such a sum accomplish, if laid out for the education and improvement of the young of our country!

It is truly melancholy to witness the great number of the young who use tobacco now-a-days. It is painful to contem-

plate how many promising youths become stunted in their growth and enfeebled in their minds before they arrive at the age of manhood. A recent writer administers a strong and wholesome rebuke to boy smokers and chewers, assuring them that tobacco has spoiled and ruined thousands of boys, inducing a dangerous precocity, morbidly developing the passions, softening and weakening the bones, injuring the brain, spinal marrow, and entire nervous system. A boy who early and frequently smokes, or in any way uses large quantities of tobacco, never is known to make a man of much energy of character, and generally lacks physical and muscular, as well as mental energy. It does not tend to develope true virility or to hasten manhood, though many boys think it does, as they puff and chew in imitation of their elders. It does not prolong life, but shortens it. I would particularly warn boys, who want to be any thing in the world, to shun tobacco as a most baneful poison.

The influence of tobacco on the mental powers is beginning to excite attention all over the world. In Paris, where the habit of smoking is so generally prevalent among French students, it has been observed that the collegiate standing of those addicted to its use is much inferior to that of the students not so addicted. In the various competitive examinations, the smokers have proved themselves far inferior to the others. Not only is this the case in the examinations on entering the school, but in the various ordeals that they have to pass through, in a year, the average rank of the smokers had constantly fallen, while those who did not smoke were found to enjoy a cerebral atmosphere of the purest kind. Like investigations made in this country have resulted in similar conclusions. Recent examinations have shown a large per centage of intellectual vigor in favor of those who do not use tobacco,— too large, to consider it merely accidental.

Dr. Waterhouse, of Cambridge, in a lecture, fifty years ago, painted the sad inroads then being made upon the health of the students of Harvard by tobacco, used in various forms. It is difficult to suppose what he would say if he could come up now, and see the extent of the practice in our colleges. That the evil is increasing, may be inferred from the increasing number of

feeble, sallow and haggard young men. In this city these results are apparent. There can be no doubt our young men are not as robust and strong as they were fifty years ago. More thin, pale, sallow youths are met with; and when we see what numbers of young men have become virtually slaves to this nauseous drug, we need not be surprised that these effects are produced. I fear the result if matters go on at this rate. I fear that, fifty years hence, this enervating, destroying vice will seriously impair the integrity, the health and the morals of the race! What it has done can be seen all around us. Is not the same cause rapidly increasing, and will it not produce still more generally disastrous effects? A young father does not know what seeds of debility and disease he implants in his offspring by this habit, nor think of the evils of his parental example!

Since tobacco is sedative-narcotic in action, it necessarily controls and influences the functions of the brain, diminishes its force and activity, awakens its powers, and renders the consumer entirely unable to accomplish great mental achievements.

I am truly sorry to observe that tobacco is habitually used by some females, as well as males. We had a right to hope and expect their delicate habits would have refused the use of so nauseous a weed; but, alas! our hopes were those of benevolence. With too exalted notions of female delicacy, blended with a natural respect for the sex, we might anticipate they, at least, would join us, in striving to put down so detestable a custom, and especially never allow tobacco in any form to pass their lips. Experience and observation, however, prove the contrary. It is a custom too prevalent, among certain classes, to use snuff as a dentifrice; and though I am very loth to acknowledge it, the daughters of Carolina are not entirely exempt! Its influence can not be otherwise than highly pernicious. The women of the aborigines believed that tobacco was not only injurious to the body, but also tended greatly to diminish the "venereal appetite," and prevent *conception;* hence, it is said, they forbore its use in any form whatever.

We have thus far seen that tobacco alone, when inveterately used, is highly injurious to the system; but when virulent poi-

sons are added to its various preparations, we may be assured its noxious qualities become greatly augmented. The dishonest manufacturer frequently employs arsenious acid, strychnia, &c., to render more saleable and durable his products. A manufacturer once acknowledged he used these articles, in preparing his twist, to guard against moisture! Others have confessed to having resorted to other means, perhaps less injurious, but certainly highly disgusting. Thus, it is said their stock is frequently laid in "bog-houses," so that, becoming impregnated with the volatile salts of the excrements, it may be rendered stronger and more palatable!

A gentleman, whose curiosity led him to see tobacco-pressing, observed that the boys, who prepared the leaves for the press, had a vessel of *urine* standing by, with which they moistened them! Do tobacco consumers know this, and yet continue their most disgusting practice? Can any person think of the above composedly, with a quid in his mouth?

I have endeavored thus far to prove that this habit, when excessively indulged in, is liable to produce, constitutionally as well as locally, the most dire effects. Let us now briefly consider the most common forms of disease engendered by its abuse.

The entire system suffers; the nervous system becomes greatly impaired; the general health is injured; life is undoubtedly shortened; the vital functions become so weakened, that the body is rendered more liable to be injured by noxious agents, and is not so well able to resist the ravages of disease. The following are some of the maladies most liable to be induced:—an excessive perverted nutrition; dyspepsia; emaciation; ulcerous and cancerous formations; neuralgic pains of the entire body; apoplexies; blindness; paralysis; epilepsy, and not unfrequently insanity. The instances of a perverted nutrition are abundant. Extended observation has proven that it does not impart any degree of nutrition to the body, but, on the contrary, that it tends greatly to diminish obesity; yet such is its stimulating qualities, that it is capable of supporting the system for several successive days above what may be termed "the hungry point." In this respect it much resembles opium and alcohol, yet is en-

tirely different in its operations. To this stimulating quality is probably attributed the various virtues assigned to it by prejudice and superstition.

A European writer, Dr. Waynwaring, in a treatise on Scurvy, has written largely against the use of this herb, and asserts, in the most positive manner, that it is a grand producing cause of scorbutic complaints, and that scurvy has abounded much more, in all nations, since the introduction and use of tobacco, than ever before. Others testify in like manner.

Dyspepsia, when caused by tobacco, which it frequently is, is characterized by the most urgent symptoms peculiar to that disease. Half the diseases that torment and destroy mankind have their origin from a bad digestion; and since I have previously endeavored to establish that the use of tobacco injures that function, I infer that many of the worst forms of disease have here their origin, inasmuch as its use depraves those juices, without which good digestion is impossible. One of the first effects produced by indigestion is flatulency, the most obstinate and incurable kind arising from this cause. Diarrhœa is also a frequent symptom. The bowels become so loose and irritable, that the least invasion of cold, or change of diet, causes much suffering and inconvenience; hence the emaciation and decay so generally visible in the dyspepsia of inveterate tobacco consumers. The stomach and bowels having lost all their sensibility, the countenance assumes an anxious, haggard, nervous expression, the skin assumes the appearance of old parchment, the heart pulsates feebly and irregularly, and the mind well nigh ceases to receive any impression.

The local effects are not less frightful or fatal. Horrid ulcers and cancerous formations are said to be occasionally induced in the upper portion of the alimentary canal. The mouth, the pharynx and œsophagus, are lined with a great number of ramifying nerves, very sensible to irritation, and in structure as delicate and sensitive as those of any other mucous membrane of the body, if not even more so. The mucous membrane of these parts is not only lined with delicate nerves, but its inner coat is glandular, and since many vessels open here, these parts can not but receive

a great quantity of the tincture of this herb before it enters the stomach. The injuries these parts receive by the intense acrimony of this tincture is sometimes frightful, and are in part out of reach of art, being too far removed from the surface of the body to obtain relief from the surgeon's hands; and mere medication, in such cases, can be of little or no avail. Especially is this the case in ulcerations of the æsophagus.

An instance is related of a lady, greatly addicted to the use of Scotch snuff, which finally gave rise to a fleshy tumor in the æsophagus, preventing her taking any solid food. For several years she subsisted on slop diet, which she sipped through a quill, but at length became so exhausted, that she sank and died of emaciation.

In the same manner ulcers and tumors of the mouth and pharynx have been produced.

Cancerous formations have frequently origin here. The tongue and lips appear to be especially liable to become affected. M. Bouisson, of Montpelier, in "A Blast Against Tobacco," deduces the increased prevalence of labial cancer, from the excessive and long-continued use of tobacco. He relates sixty-eight cases affected with cancerous tumors, in persons addicted to smoking, and then traces a complete pathological history of the disease. I will give a brief *resumé* of the arguments he employs to prove his etiological doctrine, remarking, however, that they apply rather to a cancroid condition, than to cancer, properly so called. "There is no doubt," says he, "that cancer of the lip, and other parts of the mouth, has become more frequent in proportion to the increased numbers of smokers, and to the increased amount of smoking, among those who had already contracted the habit. This lesion was quite rare in the last century. If we examine the works of the surgeons at that time, and even of those of the beginning of the present century, we find that cancer of the lip is not noted, as having any marked predominance over cancer of any other part of the body, and none of them seem to have suspected that tobacco could play any part in determining the disease."

All the works devoted to this disease, whether by French

writers, or those of other nations, are silent on this point of etiology; hence we are justified in inferring, that if such men as Boyer, Dellpech, Bell, Richeraud, Heister, Jourdain, &c., did not notice the frequency of labial cancer, in the proportion in which we find it at present, it was because the occasional cause of the disease was less common than now, and the number of smokers being fewer, the number of diseases which this custom engenders was also less considerable. But now the increase in the number of smokers, *à outrance*— men who are never without a pipe or cigar in their mouths,— for whom even sleep does not remove the offending weed,— this increase, he remarks, has also multiplied, in a remarkable degree, the number of cancerous diseases of the mouth requiring surgical operations. Never before was there so much talk in hospitals, and in private practice, of cauterization, of excision of the lip, and of cheiloplasty.

M. Bouisson is of opinion, besides, that something more than the mere habit of smoking is required to produce labial cancer, and properly accords a good deal of influence to a general diathesis; but he believes, moreover, that he has good reason to maintain that this morbid predisposition would remain latent for a longer period, or even entirely, but for the local exciting cause, produced by the irritative action of the tobacco, incessantly concentrated on one point. Besides the arguments deduced from the increased frequency of labial epithelioma, the author brings the following reasons to his aid: it attacks the lower lip most frequently, it being on that the pipe-stem or cigar habitually rests; and that it is rare among women and children. He, however, speaks of a woman who was affected, but she was an inveterate smoker. The older the smoker, the more common the disease; indeed, most of the cases occurring in persons of upwards of forty years. Among the poorer classes, who smoke short-stemmed pipes and bad tobacco, the disease is developed sooner than among the rich, who smoke delicate cigars and long-stemmed pipes, and, besides, neutralize the effects of the combustion of tobacco by hygiènic means. The local action of the heat upon the lips has probably some effect in the production of the disease, but the acrimony of tobacco alone would suffice.

The author gives the case of a physician of Barcelona, from whom he removed some epithelial vegetations in the nostrils, which he had no hesitation in ascribing to the practice so common among Spaniards, of eliminating the smoke of cigarettes through the nose; and it is thus, probably, that in many cases epithelial cancers of the tongue, cheeks, arches of the palate, gums and tonsils are produced.

Dr. John Lizars relates the case of a gentleman, fifty-eight years of age, of strong frame and healthy constitution, an intolerable consumer of tobacco, who acquired thereby a cancerous tongue, which gradually mouldered away, the patient perishing in the most intense agony. Another, of a middle-aged lady, also an inveterate consumer, who became alarmed at the appearance of a small wart-looking growth, in the centre of the tongue, frequently giving most intense pain, finally ulcerating, and in twelve months destroying life. These, with many others, he attributes to the habit of using tobacco.

Fleshy excrescences, or polypi, are said to be occasioned in the nostrils by the use of snuff. A great variety of instances of this kind might be adduced, but I will only mention two, as related by different authors.

"A person of the name of Francis, surgeon and apothecary, though thoroughly sensible of the many ill consequences attending the habit of snuff-taking, had not the resolution to abstain from it, but even made use of the most pungent sort he could procure. The consequence was a large excrescence, in form of a polypus, filling one of his nostrils, which affected, not his voice only, but his breathing, to a very high degree."

The other case was in a gentleman who made use of an immoderate quantity of this pernicious powder, the acrimony of which occasioned fleshy excrescences in both nostrils, which became exceedingly inconvenient and troublesome; and the patient, not being willing to submit to the hand of the surgeon for his relief, neglected them so long, that they degenerated into cancers, which, of course, ended fatally.

These are serious and important considerations, and consumers should take warning, and at least indulge moderately in the habit.

The evidences of nervous disorders are many and conclusive. Those of a so-called nervous temperament are the earliest and surest victims to disturbed sleep, an enfeebled condition of the voluntary muscles, and shattered nerves. The consumer never dreams what a long array of ills is caused by his habit. Even in the moderate consumer there is a tendency to vertigo, headaches, and various neuralgic disorders; hence, we may readily infer why the immediate consumer should become predisposed to more serious nervous derangements, as apoplexy, and even insanity. When considering the habit of snuffing, I cited the opinions of several able writers, on the tendency of this habit in inducing apoplexy, hence it is unnecessary to farther remark on this subject.

Dr. Brinsdale, late president of New York State Medical Society, stated in his address before that body, that three-fourths of the cases of amaurosis that are upon his record have occurred in those who have been in the habit of the excessive use of tobacco.

The following is an interesting case of amaurosis from smoking:—"J.W., a coachmaker, upwards of fifty years of age, had smoked for thirty years, generally two ounces of tobacco a day, when he became so blind as to be unable to work, or even walk through a crowded street. He applied to an eye-dispensary, where the medical man, who was considered a good oculist, told him he was laboring under amaurosis, and prescribed accordingly. After following his treatment for some time, and finding himself no better, he visited a neighboring city, and consulted another oculist, who instantly pronounced tobacco to be the cause of his blindness, as if the obnoxious stench of the weed had led him at once to this conclusion. J. W. instantly 'threw away tobacco forever,' visited a relative in the Highlands, when, in a short time, his vision began gradually to return, became clear, and enabled him to return to his business, entirely cured." The above is taken from Dr. Lizar's recent work on tobacco, and is an instructive case.

It is difficult to determine what the action may be in producing blindness. It would appear the nerves of the mouth and nostrils are first affected, then the brain, thirdly the nerves of

the eyes, and finally those of the whole system. At the same time, the poison being mixed and swallowed with the saliva, must be absorbed in the stomach and intestines, carried by means of the lymphatics into the circulation, and, through the blood, act again upon the nervous system, in the same manner that strychnia does.

If such serious effects may be produced upon the nerves of the eyes, and even cause blindness, why should it not produce paralysis of other nerves, as those of the arms, legs, and, indeed, of the nerves of the entire body? This indeed does happen, and that, too, quite frequently. The form of palsy produced is generally hemiplegia, and almost always incurable. In the able clinical lecture of Mr. Solly, surgeon to St. Thomas's Hospital, London, on paralysis, occurs the following sentence: "There was another habit, also, in which my patient indulged, and which I can not but regard as the curse of the present age,—I mean, smoking! Now, don't be frightened, my young friends, I am not going to give a sermon against smoking,—that is not my business; but it is my business to point out to you all the various and insidious causes of general paralysis, and smoking is one of them. I know of no single vice which does so much harm as smoking. It is a snare—a delusion! It soothes the excited nervous system at the time, to render it more irritable and more feeble ultimately. I believe that cases of general paralysis are more frequent than they used to be, and I suspect that smoking tobacco is one of the causes of this increase."

Mr. Miller, in a late treatise on "Alcohol, its Place and Power," alluding to the popular idea of alcoholic stimulants being strengthening, presents the following happy illustration: "Alcohol, to the working human frame, is as a fire to the wick of an oil lamp: with this you raise the wick from time to time, and each raising may be followed by a burst of brighter flame; but while you neither add cotton nor oil, the existing supply of both is, through such pin-work, all the more easily consumed. Genius may have its poetical and imaginative powers stirred up into fitful paroxysms by alcohol, no doubt; the control of will being gone, or going, the mind is left to take ideas as they come, and they may

come brilliantly for a while; but at best the man is but a revolving light: at one time a flash will dazzle you; at another, the darkness is as that of midnight, the alternative gloom being always longer than the period of light, and all the more intense by reason of the other's brightness. Whilst imagination sparkles, reason is depressed; and therefore let the true student eschew the bottle's aid."

Could any illustration be more beautiful, and does it not apply to the abuse of tobacco, as well as to that of alcohol?

I have always placed in the same category tobacco, opium and alcohol. In their legitimate places, they are invaluable; but by abuse they have proven a curse to our race. It is extremely difficult to determine which of these three tends most to demoralize mankind.

Dr. Brinsdale, of New York, believes that seven-eighths of the cases of epilepsy in adults (exclusive of puerperal convulsions, and those connected with injuries of the head,) have occurred for the first time in those who have been in the habit of some stimulant or narcotic, as ardent spirits, opium or tobacco.

Dr. Nebinger, member of Philadelphia County Medical Society, in a recent meeting of that association, used the following language in a discussion on the evils of tobacco, considered in their moral, social and physical influences: "In my own professional walks I have met with two unfortunate epileptics, the origin of whose sad affliction could be fairly traced to a large consumption of this poisonous weed; one of these, the offspring of healthy parents, in whom no such constitutional peculiarities could be traced, used tobacco to a great excess, chewing half a pound per week. This man's epilepsy progressed, until his mind became impaired, and an insane hospital became his home, where, but a few weeks ago, he died. The other epileptic is a poor laboring man; he was also an excessive chewer. He sought me for the treatment of his fits. Believing tobacco to be their cause, I directed him to abandon its use, which he did. I placed him upon a treatment, a part of which was the administration of nervines, and I had the pleasure of seeing him, after a long perseverance, completely recover; I say completely recover, for several years

have now rolled by, and he has not had a return of the epilepsy."

While we thus read what has been the experience of eminent practitioners, we cannot but discover that their observations coincide most remarkably. Insanity is a fearful result of the excessive use of tobacco. We have, alas, too many well-authenticated instances where this worst of human maladies has been thus induced. I have previously remarked, that the terrible ravages which tobacco is making on the minds of the young seems to be attracting the attention of the world.

In a letter written by a distinguished citizen of Hartford, Conn., to the Rev. Dr. Hawes of that city, occurs the following observation: "My first room-mate in college, a cousin, and the son of a distinguished divine in Philadelphia, was for many years a maniac, and afterwards an idiot, solely in consequence of his inordinate use of this vile weed. And from how many thousands of our young men does this poisonous narcotic steal not only money, health, comeliness of feature and complexion, and cleanly and gentlemanly habits, but all moral elevation, sensibility of conscience, and energy of noble purpose — to leave, instead, an enfeebled intellect, disordered nerves, and a polluted body and mind!"

In the report of 1850, of the Maine Insane Asylum, occurs an article, proving the influence of tobacco in producing insanity.

In a pamphlet recently issued by Dr. Seymour, of London, on Private Lunatic Asylums, and the causes of insanity of late years, the Doctor denounces with emphasis, as one of the producing causes, the practice of immoderate use of tobacco, indulged in by boys and young men, at the universities and large schools, now called "colleges." The Doctor's remarks are very opportune, and are as applicable to the youth of this country as of Europe.

Mr. Lizars mentions two amiable brothers, who had, in consequence of habitual use, gone deranged, and then committed suicide. There was no heriditary predisposition to insanity in their family.

A general determination of morbid matter, or of acrimonious, stimulating particles, settling on the brain, has from all ages

been accounted capable of producing the worst forms of insanity; therefore tobacco, a morbid, acrimonious, stimulating substance, has been reputed, when excessively used, either by smoking, chewing or snuffing, one of the exciting causes of this dread malady.

There are many cases on record affirming the injurious influence of tobacco on the genital functions. It is difficult to conclude whether this influence be due to the action of this narcotic on the cerebellum alone, the cerebro-spinal system, or directly upon the spermatic organs. The symptoms are at first generally those of narcotic poisoning, followed by persistent headache, chiefly referable to the cerebellar region. Physiologists have long been in dispute as to the functions of the cerebellum, yet I am of the opinion that the situation of headaches in genital debility, universally referred to this region by authors, is a powerful argument in favor of those who believe this organ to be more or less concerned in the functions of the genital organs. However this may be, we have abundant evidences that tobacco is liable to produce emasculation, a result, perhaps, more terrible than any yet adverted to. It is generally known among medical men, that the venereal desires of smokers diminish as the habit of smoking increases on them.

The action of tobacco on those who use it for the first time is too well known to require description. There are many who will readily testify what they experienced when endeavoring to accustom themselves to its use. These effects go off by degrees, as the consumer becomes habituated, and may cease altogether. But the custom has its limits: the evils will reäppear if indulged in to excess. Thus it is that the most inveterate consumers often experience vertigo, headache, nausea, &c., without once dreaming of the exciting cause. I do not mean to imply that all will feel such ill effects, but I do assert that the tendency is unquestionably in that direction. Many may escape for awhile (thanks to their greater powers of constitution for resisting disease and death!), just as some persons for a time continue to drink liquor or use opium, without feeling bad effects; but in every case the poison is in the system, and must be doing its work.

I can not forbear briefly to treat of the poisonous qualities of tobacco, when taken in decided doses. As the subject is avowedly of the highest importance, and one on which every one concerned in medical practice can not have too clear and prompt ideas, it will not, I think, be an unprofitable labor if we take a general view of the nature and effects of its preparations, which, when given even in comparatively small doses, interrupt and destroy the vital functions; and though it be too true that their action is so sudden, so violent and so deadly, as frequently to render every effort to save life fruitless, it is essential, and may be useful to know, how rapidly and in what manner death supervenes. In substance, tobacco is not as frequently a cause of death, as are some of its preparations. Especially is this true when taken internally, for, by virtue of its emetic properties, the stomach generally expels it immediately. In form of enemata and cataplasms, however, death has been known to result, and several cases are recorded: one, that of a boy, to whose head the juice of the fresh leaves was applied, for the cure of *tinea capitis*, is said to have died in three hours, notwithstanding the most energetic treatment. Another case, where a physician was sent for to see a girl of seven years, grievously tormented with spasms in the stomach, and excessive vomitings. Various means of relief were resorted to, but without success, neither could the cause of her attack be discovered for some time: at last, however, the strong smell of tobacco led to the detection. She had also been suffering with *tinea capitis*, and her mother, to cure it, and cleanse the head, had that day applied an ointment of butter and snuff. This was found to be the sole cause of her violent symptoms, which had nearly put an end to her existence.

We have it related where, by way of experiment, three drops of *nicotianin* were administered to a large dog. Shortly after he began to look violent and furious—lolled out his tongue, worked his flanks, and manifested much general uneasiness. The nerves became violently contracted; rigidity became so great, that the dog, when attempting to walk, recoiled backwards, and fell into convulsions. In this state he continued for twenty-four hours, after which time, though very feeble, he recovered.

A drop of the *ol. tabaci* put upon the tongue of a cat is said to have induced violent convulsions, and caused death in the space of one minute. A small portion placed into a wound, in a leg of a pigeon, killed it in two minutes. An ounce of the infusion injected into the rectum of a cat produced slight convulsions of the abdominal parietes, and a great degree of general disquietude, for half an hour. A copious evacuation succeeded these symptoms, which lessened them considerably: at the end of an hour they were evidently diminished, and every appearance totally subsided in the course of a few hours.

These experiments go to prove the poisonous properties of the chief constituent principles of tobacco, and should forever quiet any doubts relative to the injurious sequences of habitual use. It is to be hoped such an accumulation of evidence as at the present day exists will have a decided tendency and effect in diminishing the use of this most noxious weed. Intelligent consumers should at least lay it aside, thereby setting an example to the more ignorant, who alone can be influenced by direct example. The great difficulty of eradicating prejudice from their minds is too well known, and, though much to be regretted, friendly admonitions are too frequently received in an improper spirit. Hence, for permanent and effectual remedy, we must look to the intelligent, and especially to the members of the medical profession, upon whom it devolves, as a solemn duty, to raise their voices and cry aloud against its abuse, not only because of the physical and mental debasement, but the moral degradation which springs up, so detrimental to civilization and republican institutions.

In conclusion, I would say to those—and especially the young—not yet initiated into this habit, Never begin. No good can possibly come from it; but, contrarily, as I have striven to prove, harm will. It will injure your health, your integrity, your morals, your standing in society. You are now free; do not enslave yourselves. Let the habit alone—have nothing to do with it.

To the moderate consumer I would say, Desist; stop where you are, entirely and forever: you can not say one word in defense of it. Ask any intelligent consumer to give one valid reason

for its use, and he will unhesitatingly confess he can not do it. It does shorten life, makes you a slave to sensuous habits and appetites, debilitates and ruins you.

To the habitual and confirmed consumer I appeal, though with little hope of effecting a reform. I know the struggle will be dreadful, but it is hard because the vice takes away your will. Break asunder, *now*, the chains that bind you; better do it now, than never!

Happily for every one that tries, he has powerful allies to assist him. Against Tobacco are marshalled, Woman; the Constitution of Man; the Voice of Nature; the Hopes of the Race.

TO THE EDITOR OF THE MEDICAL JOURNAL OF NORTH CAROLINA.

BALTIMORE, JULY, 1860.

Dear Sir:

As you have recently received an appointment in the University of Maryland, and purpose to reside for the future in the Monumental City, I have thought it not inappropriate to give you some idea of the advantages which Baltimore can offer to such students of Medicine as may honor her with their patronage.

The school of Medicine in the University of Maryland is one of the oldest in the country. It has already passed its fifty-second birthday, and "will commence its fifty-third session on the 15th of October next." At one period it ranked, in point of numbers, with the largest of our medical schools, and was universally esteemed one of the first institutions of its kind in America. An unfortunate dispute, however, between the Faculty and Trustees, as to the extent of their respective prerogatives, eventuated in a protracted lawsuit, and for a time closed its doors to the public. This controversy was ultimately decided in favor of the Professors; but when they resumed their labors, they discovered that the rivalry of opposing schools, or some other cause, had directed the tide of students in other directions, and that they were compelled to deliver their lectures to largely diminished classes. Not daunted, however, by this drawback, they devoted themselves with redoubled energy to their duties,

—enlarging their accommodations,—increasing their facilities for hospital instruction,—filling up their ranks with young and able men,—adding to the number and completeness of their specimens, models, plates, and anatomical illustrations,—and, in fact, keeping pace in every particular with the progress of science, the demands of the age, and the obligations imposed by their responsible positions as teachers of the Medical Art. As a reward for their labors in this regard, they have had the satisfaction of seeing the number of their students gradually, but surely increase,—of sending out graduates who have reflected the highest honors upon their instructors, and of having their school take a most commanding position among the medical institutions of this country, so that at the present time, if it has not come up to the full measure of its former greatness, there is abundant evidence that its march is onward, and that a noble destiny awaits it in the future.

The University of Maryland has always had an able Faculty. The names of Dunglison, Potter, Power, Thomas, Frick, and others of equal distinction, adorn its past annals; whilst its chairs are even now filled by such men as Smith, Miltenberger, Chew, Aiken and Hammond,—than whom no other school in America can boast of a more *élite* corps of Professors. Comparisons may be odious, but I can not help referring to the pre-eminent position which is occupied by Dr. Nathan R. Smith, throughout the whole medical world, as an able and successful surgeon. If you will refer to modern works on surgery,—such, for instance, as Gross' great Book, and Hamilton's admirable Treatise,—you will find that no American has done more for this department of the profession, in the way of useful and ingenious instruments, appliances, &c., than the Professor of Surgery in the University of Maryland. If you will observe the statistics which have been collected of successful capital operations in this country, you will ascertain that, for the number of cases operated upon, no one has been more fortunate or skillful than this distinguished gentleman. If you will travel abroad,—and I speak from experience,—and inquire of the savans of Europe who they regard as the most eminent and able of American surgeons, you

will learn, that next to Dr. Mott, of New York, they rank Doctor Smith, of Baltimore. Indeed, wherever true professional skill and high scientific merit are appreciated, there this great Southern surgeon has multitudes of friends who are ready to accord to him the honor which is due to his exalted position and preëminent ability. No one can be more dextrous in the use of the knife, or successful in the adjustment of such appliances as belong specially to his department; and it is the unanimous opinion of such as have attended his clinical instructions, that more of practical knowledge can be learned from him in one half hour, at the operating table or the bedside, than by a dozen lectures from any ordinary, or even extraordinary Professor.

I do not hazard any thing in saying, that, as a surgical school, Baltimore can compare favorably either with Philadelphia or New York. It is true that the hospitals or clinics of these cities offer a great number and variety of cases; but it is equally certain that Baltimore affords *ample* material for the successful prosecution of the study of surgery in all of its branches; and as it has always been regarded as a medical truism, that *enough* is better than a *surfeit*, it follows that, in this particular at least, the school has the advantage of its rivals.

Having thus introduced the Infirmary, I beg leave to refer to it particularly, as I feel proud of it, both as a Baltimorean and as a friend of the University of Maryland. This institution, which contains one hundred and fifty beds, independent of the department exclusively devoted to private patients, is in immediate proximity to the College, and under the *special supervision of the Faculty* of the College. It is a large and commodious hospital, affording ample accommodations for the number of patients above mentioned, supplied with the most excellent nurses, and offering to the students such facilities for clinical instruction as can be presented by few institutions of a similar character in this or any other country. It is true, as I have remarked in another connection, that the number of patients annually received in this hospital is less than that subjected to medical treatment in some of the larger institutions of the North; but, with the exception, perhaps, of New Orleans, which is too far South to prove attractive

to students generally, it will compare favorably, in this regard, with any institution out of the large cities of the North. Besides, you know well that the faithful study of a few leading cases is really of more benefit to the student than the hasty and incomplete observation of a large number of patients; whilst your observation of the manner in which clinical instruction is given, both at home and abroad, must have convinced you of the utter impossibility of learning any thing of any decided value when professors are embarrassed by the presence of overgrown classes, and the necessity for prescribing for too great a number of patients within the circumscribed limits of the time usually devoted to bedside teaching. Michelet, the French historian, and the inimitable author of "L'Amour" and "La Femme," declares, that his knowledge of the Latin language was acquired from the faithful study of one single book, Virgil. Dr. Drake, the great medical philosopher of the West, was in the habit of bringing a single individual before his class, and illustrating his views of therapeutics and pathology upon him, either by the administration of medicines or the artificial induction of disease. And it is everywhere recognized as an axiom, that neither mind nor body is capable of assimilating more than a definite amount of pabulum in a given time, and that the attempt to induce hyper-nutrition eventuates, as a matter of necessity, in the development, not of augmented strength and redundant health, but of positive pathological conditions in the economy. From these facts, the inference is legitimate, that the careful study of a few well-selected cases does really prove of more benefit to the student than the hurried and improper digestion of the immense mass of heterogeneous material which is supplied by the "great schools" and the "large hospitals," to which the junior disciples of Æsculapius are continually invited with such a "flourish of trumpets" by a disinterested (?) Faculty. As an example of the advantages which the Baltimore Infirmary offers to students, let me refer you to the subjoined list of diseases treated in its wards during the last year:—

Medical Diseases.—Abortion; Angina Diffusa; Aphonia; Apoplexy; Bright's Disease; Bronchitis; Chorea; Cirrhosis of Liver; Colica Pictonum;

Delirium Tremens; Dementia; Diabetes Mellitus; Diarrhœa, Diuresis; Dropsy—Anasarca, Ascites; Duodenitis, chronic; Dysentery; Dyspepsia; Emphysema of Lungs; Enteritis, acute, chronic; Epilepsy; Fever—Intermittent, Remittent, Typhoid, Yellow; Gastritis, chronic; Heart, functional disease of, hypertrophy of, valvular disease of; Hemicrania; Hepatitis; Hydro-pneumo-thorax; Hysteria; Metritis; Neuralgia; Parturition; Paralysis, Facial, Hemiplegia, Paraplegia; Peritonitis; Phthisis; Pleuritis; Pleuro-Pneumonia; Pneumonia; Prolapsus Uteri; Renal Cysts, with Dysentery; Rheumatism, acute, chronic; Rubeola; Sciataca; Scorbutus; Softening of Brain; Spermatorrhœa; Struma; Tonsillitis; Tetanus, chronic.

Surgical Diseases.—Abscess; Amaurosis; Aneurism; Anthrax; Arthritis; Arthrocace; Burns; Calculus, renal, vesical; Caries of Ribs, Tarsal Bones, Vertebræ; Cataract; Conjunctivitis, acute, chronic; Contusions; Cophosis; Cystitis; Eczema; Entropion; Epulis; Erysipelas; Fistula in ano, lachrymalis, urinal, recto-vesico-vaginal, vesico-vaginal; Foreign body in bronchus, in œsophagus; Fracture of Clavicle, of Cranium, of Femur (simple), of Femur (compound and comminuted), of Fibula, of Humerus, of Metatarsal Bones, of Patella, of Radius, of Radius et ulna (compound and comminuted), of Radius et ulna (compound), of Ribs, of Tibia, of Tibia et Fibula (simple), of Tibia et Fibula (compound and comminuted), of Ulna, of Vertebræ; Gonorrhœa; Hemorrhoids; Hernia, inguinal; Hydrocele; Hydrosarcocele; Iritis, acute, syphilitic; Leucorrhœa; Lichen; Luxation of Humerus; Necrosis; Onychia Maligna; Ophthalmia, gonorrhœal, strumous; Orchitis; Ozæna; Paronychia; Pernio; Periostitis; Polypus, nasi, uteri; Procidentia Iridis; Psoriasis; Schirrus; Staphyloma; Stricture of Urethra; Sycosis; Syphilis; Synovitis; Tinea Capitis; Trichiasis; Tumors; Ulcers; Wounds, gunshot, incised, lacerated.

Surgical Operations.—Amputation of Arm, of Hand, of Fingers, of Femur, of Toes; Autoplastic operation; Cataract; Entropion; Extirpation of Mammæ, of Tumors, of Tonsils; Fistula in ano, lachrymalis, vesico-vaginal, operations for; Hernia, inguinal, operation for; Hydrocele, operation for; Lithotomy, operation for; Lithotripsy; Necrosis; Paracentesis abdominis; Removal of Polypus nasi, uteri; Reduction of dislocated humerus; Staphyloma, operation for.

You must remember, in this connection, that this Infirmary is under the *immediate* control of *the Faculty* of the University, which is an *important fact*, as it secures to its matriculates such privileges in this regard as can be enjoyed in but few of our medical institutions. During the Session of the School daily instruction is given at the bedside by the Professors of Surgery and of the Principles and Practice of Medicine, whilst the same system is continued throughout the year by other members of the Faculty, for the benefit of all who may choose to attend. In addition to this, ten students are permitted to remain in the In-

firmary, as "Clinical Assistants," and as these places become vacant annually by the graduation of their occupants, there are occasions constantly presenting themselves for the advancement of those whose conduct and acquirements merit such distinction. Taking all these things together, I think you will agree with me, that, as regards clinical instruction, the University of Maryland stands *primus inter pares*, and that its advantages in this particular deserve the most attentive consideration of all who are engaged in the study of Medicine.

There is another consideration, of no inconsiderable moment, which should be remembered here: Baltimore is a Southern city, whatever may be said to the contrary. I do not refer to her geographical situation or political connections, though, as one of her humblest citizens, I must assert her loyalty and devotion to the interests of the South; but I mean to say that, in a medical point of view, as regards the classes of persons who come under the supervision of her physicians, and the nature of the affections which they are called upon to treat, she is essentially Southern. No man who has received his education exclusively at a Northern school can be prepared to practice Medicine to advantage at the South, for two reasons, which must be apparent to every reflecting mind. The first of these consists in the fact that a large number of the patients Southern practitioners have to treat belong to that class of the population which is comparatively scarce at the North,—I refer to our negroes. All who have had experience in these matters know, that between the blacks and the whites there are vast differences, both as regards the course and character of the diseases which affect them, and their susceptibilities to the action of remedial agents. Northern graduates have to learn all these things at the bedside, after entering upon their professional duties, whereby, of course, much time is lost to them, and perhaps a considerable amount of property destroyed for their patrons. Southern graduates begin the practice of Medicine with these important facts well indoctrinated into them, —with a full knowledge of those important distinctions which sound pathology demands shall be drawn between the two races; and it must be palpable to all unprejudiced minds, that, so far as

the treatment of negroes is concerned, they have a decided advantage over physicians from other localities. Maryland is a slaveholding State, and in Baltimore there are still a great many negroes. Besides this, our city is in such close proximity to Virginia and North Carolina, and the fame of our physicians is such in those sections of the country where blacks are numerous, that numbers of them are annually sent to the Infirmary for medical treatment. The black wards — for there are two of them — are large and commodious, and furnish the most admirable opportunities for the study of such maladies as pertain to the negro race. Here it is that the student is taught to observe and distinguish all the peculiarities which belong to this class of the population. {Here it is that those who are to pursue their profession in the South are supplied with that peculiar knowledge in regard to the predispositions, proclivities and susceptibilities of the two races, which can not be acquired save in a Southern school, and whereby the graduates of such institutions are prepared in the premises for the proper discharge of their duties. Let the man who expects to practice among negroes learn something of their diseases before his diploma is acquired — before he offers for practice, in order that the life and property may be saved which must, of necessity, be sacrificed whilst the lesson which should have been taught in the premises is being learned by experience; and let those who are interested in this species of property remember that they but jeopardize their most essential interests, when they prefer the graduate of a Northern school to one who has acquired his degree where the "diseases of negroes" were made an important part of the curriculum.

The other reason to which I have alluded is this: a majority of the diseases which a Southern practitioner is called upon to manage are modified by certain agencies which originate in the particular locality where they occur, and are peculiar to it. As an example of these agencies, let me refer briefly to malaria. It is well known that a certain subtle influence is generated in particular sections, which not only directly produces a certain group of symptoms, when the organism is brought in contact with it, —such as belong to intermittent, remittent and congestive fever,

but which so incorporates itself into the system as to impress upon it a character *sui generis*. It thus becomes the predisposing cause of many disorders, as well as the indirect agency by which multitudinous modifications are affected in the symptoms, character and progress of the diseases which may invade the system under such circumstances as these. In a word, "the malarial law," as it has been denominated, is the prolific source of a host of changes in pathological phenomena, and a source of incalculable embarrassment even to those who are most familiar with its operations. The idea that it can be properly understood by studying text books, or learned from the occasional cases which present themselves in northern latitudes, as its illustrations, is perfectly absurd. It must be carefully and continuously observed where its manifestations are most numerous, and its operations the most potent. It must be explained by those to whom an intimate, protracted, practical acquaintance have given a positive knowledge of it in all its Protean phases. It must be met, not by proxy, but face to face by him who would learn its modes of attack, and acquire the skill to effect its mastery. Or, in plain English, it must be studied in a Southern institution, under the direction of Southern teachers, and on Southern soil, before it can be understood; and all knowledge which is acquired under other circumstances than these is necessarily theoretical, and utterly useless as far as the actual practice of Medicine is concerned.

Of course, I do not mean to say that malaria is produced universally or exclusively at the South, for exceptions present themselves in both directions; but still, it is generally acknowledged, that a large majority of the diseases which present themselves there are either directly or indirectly connected with this peculiar agent, and to some extent modified by it. To such an extent is this true, that I have no hesitancy in affirming that a practical knowledge of its operation is essential to the success of nine out of ten of those who pursue the profession of Medicine on this side of "Mason and Dixon's Line;" and the medical student is blind, not only to his own interests, but to those of his patrons, who disregards so important a desideratum.

Baltimore, among other things, can boast of a rapidly extending commerce with the Southern States. Virginia, North Carolina, South Carolina, Georgia and Florida, buy largely of her manufactures and importations. Their merchants throng her streets, their sailors crowd her wharves, and their diseases congregate in her hospitals. Where, then, let me ask, can their medical students find a better institution in which to pursue their studies, than the University of Maryland? Let them but pause in their rapid flight to the inhospitable and unfriendly North, and examine the advantages with which Baltimore would tempt them to remain among her friendly and fraternal people, and, my word for it, your classes would compare with those of the "old *régime.*"

Baltimore has had the reputation of being badly governed. With the past I have nothing to do; but of her present condition, and future prospects, let me say a single word. Instead of a police force virtually appointed by those who most required control, and holding office only at their mercy, we have an efficient corps selected by the State authorities, but composed equally of both political parties. Instead of the twenty voting places, and the consequent crowding of the voters together, we have now over sixty precincts, and so scattered over the city as to give to every citizen "free scope and verge enough" for the exercise of the elective franchise. The fire department has been reorganized, and is now so well managed, that the shameful rows which once disgraced it will be prevented for the future; and the whole city, in fact, in all that concerns her governmental policy, has been thoroughly and successfully reorganized — much to the delight of her law-abiding citizens, and to increased security both of life and property within her limits. As an evidence of the improvements which have taken place in consequence of these various reforms, I need only say, that the delegates to the late political conventions in this city expressed themselves as much gratified with the order and decency which prevailed at all times and places in Baltimore, and declared, both publicly and privately, that they had never seen a better regulated city. You know, also, that when the Japanese visited us they were insulted

by none of those scenes of riot and rowdyism which greeted them on their arrival in other localities. There can be no doubt about the fact, that the city of Baltimore has been the theatre of much that was disgraceful; she has been the victim of a vast deal of malicious exaggeration; and her citizens, appreciating the extent to which their interests were being sacrificed by the bad reputation of the city, have arisen in their might, and thoroughly reformed it.

Baltimore, as regards population and commercial importance, ranks among the first cities of America. Her limits are being daily extended; her population is constantly augmenting; her commerce is growing with unparalleled rapidity; her railroads are stretching themselves out in every direction; her schools and colleges are being multiplied in number, and increasing in reputation, and she is fast becoming the New York of the South. She offers now all that the student can desire, either medically or otherwise; and she will present, in the future, every thing of improvement and attraction which can be found in the largest cities of America and Europe. Let but Southern men encourage her as they have of late, and a most magnificent destiny is before her—a destiny which will include the triumphant success of her time-honored University.

You must pardon the personality, and, perhaps, presumption of the assertion, that North Carolina is under a most special obligation to patronize the University of Maryland. You know, sir, that when a vacancy had existed in her Faculty but two weeks—a vacancy which had been created by the death of one of her most distinguished Professors,—she disregarded the claims and qualifications of many of her distinguished alumni, and filled it promptly by the appointment of one of Carolina's youngest sons. This was a compliment of the first order to the "Old North State," and surely she will appreciate it properly. Thus was she honored through one of her children, and I know that she will reciprocate the favor. A Carolinian—"a native and to the manor born"—has been placed in a position of high distinction and responsibility,—his fellow citizens are called upon to sustain him, and to encourage those to whose partiality he owes

his elevation, and I have greatly mistaken their character if they do not respond to the patriotic appeal. But "*nous verrons*," and I shall watch the next year's catalogue with much anxiety, as I do not desire that the exalted opinion I have entertained for twenty-five years of North Carolina's unpretending, but unswerving devotion to the interests, the rights and the honor of her sons shall be abated "one jot or tittle" by any act of hers.

You must really pardon me for troubling you with so long a letter, but I have been so much interested in the subject, that I have unconsciously written too much. It will afford me much pleasure to extend to you the "right hand of fellowship" when you reach our city, and to renew the pleasures of "auld lang syne."

Yours, very truly,

A BALTIMORE M. D.

SELECTIONS.

Practice.

ON MALARIAL PNEUMONIA.

BY O. F. MANSON, M.D.

"O quantum difficile est curare morbos pulmonum!
O quanto difficilius eosdem cognoscere!" BAGLIVI.

LIKE an intruder into the precincts of some sacred temple, I approach the grand theme, Pneumonia, enshrined as it is by Talent and consecrated by Genius, with mingled emotions of distrust, humility and awe. Whilst viewing the prolific productions of contemporaneous writers alone, it would seem that only a scanty gleaning could be made from the apparently exhausted field; but when, in addition to these, we contemplate the vast labors and researches bequeathed by the mighty dead, the offspring of the greatest minds which have adorned our annals, it seems like a presumptuous daring to venture on the hallowed ground, and a voice, as in warning, seems to whisper the adjuration of the poet —

"Heroes have trod this spot, 'tis on their dust ye tread!"

Yet, if considerations like these had ever usurped a controlling power, Science would surely have been a loser. As the lowly miner may pluck from its secret hiding-place a gem worthy to deck an imperial diadem, so the humblest searcher into Nature's mysteries may often elicit truths which have escaped the vigilance of his more gifted compeers.

In sketching this disease, however, it is neither my aim nor expectation to produce any thing striking or original. It is my design to place upon record a simple description of its most conspicuous features as they are presented, as far as I am capable of observing and describing correctly, with my views of its nature, together with that plan of treatment most successful in its results. It is hoped that these efforts will not be entirely unattended with benefit, however slight, for, after all the thought and toil expended, the disease still often marches onward, bearing innumerable victims in its progress. In defiance of the inventions of the Materia Medica, and pathological developments; despite the tactile test of Avenbrugger, or the glorious discoveries of Lænnec, by which Science, like the Peri, before the "half-opened portal" of Paradise, listening

> "To the springs
> Of life within like music flowing,"

is enabled to detect disease in their discord, we have still too often, with Baglivi, to lament the imperfection of the principles of diagnosis and the inefficiency of therapeutic art.

It can not be doubted but that great confusion exists at present in regard to the various forms of pneumonia. It perhaps has occurred to every experienced physician, to have seen them all included in the same category, and treated in the same manner; errors to be deeply deplored, attended as they often are by the most disastrous consequences. Inflammation of the lungs, like inflammation of any other structure, may be altogether different in its nature, complications and terminations; and though technically included under the same term, its various forms require as totally opposite modes of management as the most dissimilar diseases of organs the most remote in situation and unlike in function. One practitioner, with an eye solely fixed on the lung

affection, resorts to the active antiphlogistics of sanguineous depletion and antimony, whilst another, warned by the results of such means, adopts the expectant or stimulant plan; whilst a third, led on by that spirit of generalization which has been the bane of our science, appeals to Cinchona in all cases, and under all circumstances.

Who is to blame? Assuredly, we of the South! Studying the works almost solely of British and continental writers, which, however perfect in all points for those regions, are sadly deficient for us, we force the forms of our diseases to bend to their descriptions, and blindly follow a treatment intended for varieties of disease we never witnessed. Consummate and fatal folly! Yet, year passes year only to note its repetition, whilst we, reclining in inglorious ease or censurable indifference, view the perpetuation of the error, knowing, too, that hundreds are yearly added to our ranks who will have to pass through the same trials and difficulties, and experience the same disasters we encountered, before they will be enabled to diagnose correctly, or treat successfully any diseases, save those common to all countries, without a warning, a sign or explanation from us to guide the neophyte through the labyrinth before him. Oh! awake, my friends! here is an open, unexplored path to usefulness and fame,—an object to excite ambition, commendable in its pursuit, and glorious in its attainment,—the secret of whose possession is comprised in three words—Observe! Compare! Record!

The existence of a form of pneumonia, having some undefined connection with periodical fever, has long been known and noticed; but unfortunately for the student of the present day, the affection has not attracted the attention of recent investigators, and is only cursorily and imperfectly noticed by any of the modern standard authorities. The progress of pathology, the advance of therapeutics—and still more, the change of that which Sydenham termed the Medical Constitution of the period, render it imperatively necessary, not only for the attainment of correct views of the nature of disease, but even for the recognition of its physiognomy, that its descriptions should be often revised, if not altogether rewritten. This work, to a great extent, has been accom-

plished in reference to disease generally; but as this affection is chiefly seen in regions, if not inimical, at least not fertile in authorship, the task remains (so far as we are aware) for it unperformed. To supply this deficiency is not the expectation of the writer, but he trusts that he is enabled to afford some materials from his observation, which may be of some aid to those who have not possessed his clinical advantages, and to others more competent, who may hereafter direct their special attention to the subject.

In studying the works of the great master of physical diagnosis, my attention, for several reasons to be explained, was early riveted to the fact therein related, of his having witnessed a form of pneumonia which promptly and certainly yielded to bark, whilst at the date of composing his immortal treatise he extolled, in terms as equally decided, the contra-stimulant method of Rasori, at the same time justifying his former treatment, on the ground of the peculiarity of the epidemic. With eagerness I scanned every page, to learn the distinctive points of diseases having the same name and location, yet requiring such different modes of cure. This information was more ardently coveted, as I suspected that the affection, as it was generally presented to me, belonged to the former class. The search was, however, fruitless. This important and lamentable omission is, however, easily explained by the fact that he witnessed the first-mentioned epidemic before his attention had been specially directed to diseases of the chest. Leaving his pages, I examined the productions of every author on thoracic diseases, to which access could be conveniently obtained; and although aid was received from many, towards the elucidation of the problem, yet their descriptions were so meagre, incomplete, and mingled with an exploded pathology, that I turned from them all to the bedside, to read in the volume of Nature the truths so necessary and important.

I beg those to whom these pages may come to leave out of consideration the humble source from which they emanate; to forget the faults and imperfections they may present, in the solemn assurance that the subject to which they are devoted is one of the most momentous importance to which their attention can

be directed. In my opinion the phase of disease whose delineation is now attempted, is the form of pneumonia generally seen by southern observers; that its peculiarities are often unrecognized, being often confounded with simple typhoid and other varieties of pulmonic lesion. Not only this, but the assertion is here made that, under proper management, this disease, although of decidedly fatal tendencies, is more under the control of remedial agents than any of the so-called phlegmasiæ. If these declarations are true, how important is it that its features should be familiarly known, in order that it may not be confounded with diseases chiefly affecting the same organs, but so unlike in character and nature.

GENERAL COURSE AND PROGRESS.

Malarial pneumonia attacks suddenly, being almost invariably ushered in by a chill, which is frequently of a severe intensity, and attended with rigors of variable duration. In very grave cases, however, the system is prostrated almost beyond the capability to react, and a prolonged cold stage is the result. Death, however, rarely is produced from this cause in the early stage, but fever, more or less developed, soon ensues. In some cases the patient seems for hours, and even days, to hover between the state of chill and febrile excitement. Pain in the side, of the pleuritic character, is generally next experienced, it being absent in very few instances. To the reactive fever dyspnœa and cough soon succeed. In some cases, however, the cough is tardy in its appearance, or is so slight at the commencement as to elude the notice of the patient and physician. Headache, nausea and vomiting are very frequent and prominent symptoms, and frequently precede the cough and pain, the fluids ejected from the stomach being usually of a bilious character. The cough is commonly dry at the commencement, or attended with an excretion of glairy mucus; sooner or later, however, the sputa becomes more or less tinged with blood, forming the pathognomonic ferruginous expectoration, the hues of which are as numerous as the proportions are in which blood and mucus may be mingled. Thus we shall see, in some cases, the latter merely tinged with

blood; or, as more frequently happens, a uniform thin, bloody, frothy fluid is only seen, possessing but little viscidity or tenacity, whilst in some instances a pure and copious hæmoptysis is observed. Later, however, the expectoration is liable to great variations in color and hue; thus it may be of the various shades of brown, red and yellow commingled, and in the advanced stages may become green, or almost black. The febrile action presents decided peculiarities, being usually distinctly paroxysmal and remittent. The remissions are, however, sometimes obscure, and an active fever exists during the whole day and night. On watching the patient closely, however, a coolness of the extreme parts will be almost invariably observed, generally occurring daily, and during the morning hours. This reduction of temperature may be confined to the ends of the nose, toes and fingers, either separately or combined, and in grave and rare cases may extend over the entire extremities, or even the whole surface, amounting to a state of collapse. This coolness of the skin is not generally felt by the patient, but, on the contrary, when it is present, he often complains most intensely of sensations of excessive internal heat, soliciting the introduction of cold air into the chamber, and, as we have witnessed, wishing the doors and windows to be opened, and the fires extinguished, when the thermometer stood below the freezing point near the bed of the sufferer. A marked increase of fever follows this insensible chill, which continues to increase in intensity until an exacerbation, more or less perfect, is developed. The pulse, which had been very frequent and contracted in the chill, becomes expanded, acquires force, and sometimes, though rarely, becomes full or tense. The term *compressible* applies to the usual condition of pulse, and the idea entertained on its careful examination is, that the heart is acting with only a seeming force, and not with real vigor. In some few cases, the pulse has been observed to be *nearly normal*, or *morbidly slow*.

The respiration in the chill is more frequent, and the dyspnœa more urgent than in the exacerbation. The breathing is usually irregular, on account of the pleuritic pain, but when this is absent, or has been relieved, the respiration is often but slightly

embarrassed in severe cases in the early stage. The disease continuing unchecked, the paroxysms usually become less marked, the respiration becomes more continuously frequent and difficult, the acts often exceeding sixty per minute; yet, in some cases of a grave character, the breathing is scarcely accelerated during the whole course of the disease. A new symptom now sets in. The patient, who before may have been entirely rational, or only slightly incoherent in sleep, or at the heights of the exacerbation, suddenly becomes wildly and persistently delirious. The cough now often ceases—the patient often breathing apparently better than when in a more hopeful condition. The tongue becomes brown, or black and dry; sordes collect on the teeth, and the skin and sclerotica assume a jaundiced hue. The delirium increases in intensity, the patient growing almost uncontrollable, and requiring force to keep him in bed, and when not watched, rushing out of the house when nearly pulseless and cold. Quiet only succeeds to this raving to be quickly succeeded by the curtain falling on this last scene in the agonizing drama.

Such are the usual phenomena presented by this affection, when left to the unassisted efforts of Nature. Death, however, sometimes occurs, evidently from the embarrassed functions of the lung, as in cases of the idiopathic disease, produced by an impermeable condition of its parenchyma.

The termination in health is marked by the gradual diminution of the symptoms, by the occurrence of diaphoresis, by a copious secretion of urine, and yet more often by the discharge of greenish black, viscid, inodorous stools, closely resembling tar in appearance. These passages by degrees become of a lighter color, passing through shades of green and yellow, until they assume the natural hue. The expectoration becomes less colored, the sputa becoming more homogeneous, and gradually assuming the smooth straw-colored appearance characteristic of the resolution of pulmonic inflammations.

The disease, however, sometimes becomes chronic, with copious purulent expectoration, mingled with blood, the patient passing into hectic with the rational symptoms of phthisis, terminating in death. In more fortunate cases, the disease, after

many weeks, and even months' duration, may give place to entire restoration to health.

PHYSICAL SIGNS AND PHYSICAL CHANGES, FIRST STAGE, ENGORGEMENT OR ACTIVE CONGESTION.

Advancing no claim to expertness in physical diagnosis, I feel a great hesitation in expressing my opinion in regard to the import and value of morbid sounds in this affection. I feel it my duty, however, to say that the crepitous rhonchus, regarded by Lænnec and his imitators as the earliest pathognomonic sign of pneumonic inflammation in general, is not usually present at the commencement, or, indeed, any of the stages of this disease. That it is sometimes heard, we know, but that it is so commonly observed as to possess alone any very important signification in estimating the extent, precise seat or gravity of the disease, I do not, with my present experience, believe. Its pathognomonic value, called into question at the time of its promulgation, by Andral,* Chomel,† Cruvelhier and others, has been more recently and strenuously contested by Stokes,‡ and the celebrated Professor Skoda,§ of the school of Vienna. I therefore feel more confidence in candidly expressing my views. It is difficult, after perusing the works of Lænnec, Williams, Walsh, Barth and Roger, and others, and observing the positive declarations made by them in regard to the existence and value of this sound, to withhold a belief in their statements. On the other hand, it is equally impossible to consult the distinguished authorities above cited, without being convinced that they are penned by skillful and truthful observers. I do not know how these conflicting views can be recognized, save by the supposition that their authors were observing different forms of pneumonic lesion.‖

If the peculiar variety of pneumonia now under consideration, or, if this phrase is objectionable, if the condition of the organism with which inflammation of the substance of the lungs, in

* Cl. Med., tome ii. 333. † Dict. de Med., tome xvii. 232.

‡ Diseases of the Chest.

§ Aus. and Percus., Markham's Trans., 151, 157, 303, 306, 315, 323.

‖ Lænnec's descriptions are evidently taken from cases of simple sthenic pneumonia.

the present disease, is found associated, is widely different from that described by the majority of authors, then it is reasonable to suppose that some diversity in the physical signs would be observed. If, in this disease, as we shall contend, the pathological condition of the lung usually partakes more of the character of congestion, than open inflammatory action, then the absence of crepitation may be explained, without impeaching the testimony of those who have sought to affix it to the first stage of pneumonia, as the diagnostic sound. The presence of crepitating râle is justly and reasonably ascribed to the passage of air through the viscid pathognomonic secretion in the air-cells and minute bronchi, that secretion being considered the result of inflammation. Now, although this fluid, as proven by its expectoration, is present in all varieties of pneumonia, yet it is usually more fluid, and more sanguineous in this than in simple pneumonia, and we should, therefore, rationally conclude that the passage of air through fluids of varying tenacity, density and composition, would produce sounds of a variable character.* Again, it often occurs in the early stages of this affection that the cough is unattended with expectoration for several days; indeed, we have seen cases of the most violent character, terminating in death by apnœa, with expectoration scarcely sufficient in quantity to betray the character of the affection, the cough being dry throughout, and the dyspnœa extreme. Now, in these cases we should be prepared to learn that no crepitation would be heard, or, if observed, not in proportion to the extent or gravity of the pulmonary lesion. Again, in describing the anatomical characters of pneumonia, Lænnec† admits that inflammation of the bronchia is a frequent attendant on the simple form, "its lining membrane being *commonly* very red," "occasionally swollen," "the redness sometimes extending over the whole bronchi." Now, if this be true—and who will gainsay it?—how is it possible that sonorous, sibilous and mucous rhonchi should not be often heard, as maintained

* Lænnec himself only cites two cases of this form of pneumonia in which the glutinous sputa and crepitous rhonchus were observed. (Forbes' Trans., 249.)

† Forbes' Translation, 220.

by Skoda,* and if present, drowning, according to Andral,† the less audible sound of crepitation? But, as has been premised, as the anatomical character of this affection is doubtless an intense congestion of the parenchyma, associated, perhaps, in some degree, with inflammation, it is probable that the absence of crepitation, in the large majority of cases, may be thus explained.

In the first stage of this disease we may have, therefore, the crepitating râle, or mucous râles, in varying degrees, accordingly as the character of the case may partake of inflammation or active engorgement. In many cases, however, the practitioner will fail to observe any morbid sound, although the concurrent symptoms may render the diagnosis positive. In these instances the crepitating râle may sometimes be elicited by engaging the patient to cough forcibly, as advised by Barth and Roger.‡ Although, as I believe, crepitation is neither pathognomonic—at least of this variety of pneumonia—nor confined to inflammatory affections of the parenchyma, being simply the sound produced by the passage of air through a viscid fluid in minute tubes, yet when present its importance should not be undervalued, as, when taken in connection with other signs and symptoms, it affords a valuable aid to diagnosis. There are, however, as has been already indicated, other sounds observed in the first stage of this disease. Early in the invasion, in some cases on the first day of its existence, there is a copious sero-sanguineous secretion or exhalation present, obstructing the current of air in the bronchiæ, trachea and larynx. In these instances we have, therefore, abundant bronchial, tracheal and laryngeal râles, which may be often heard at some distance from the patient. On the other hand, as we have observed, all sounds indicative of disease are absent. Dyspnœa, pain, cough, peculiar sputa, and every other symptom may be present, yet no sound save the vesicular murmur, perhaps louder than in health, is heard, the chest being at the same time clearly

* Forbes' Translation, 306.

† Note to Lænnec. (Forbes' Translation, 246.) It is traditionally well known in Paris, that even Lænnec committed the error of confounding double capillary bronchitis with double pneumonia. (Walshe, American edition, 306.)

‡ Practical Treatise on Auscultation, Edinburg edition, 141.

resonant on percussion. In these cases the pneumonia is either central, or the secretion in the air-cells and finer branches of the bronchi may not be present in quantity sufficient to offer any obstacle to the transmission of air. Whatever explanation, however, may be offered, I am fully persuaded, after careful exploration of the chest, in many cases daily and hourly made, that, at least in the early stage of this form of pneumonia, no abnormal sounds can often be observed.

SECOND STAGE—SOLIDIFICATION, HEPATIZATION.

These terms are synonymously used to denote that condition of the lung, in pneumonia, in which it is no longer permeable to the air, but doubtless are too often indiscriminately used. In this affection, as we have repeated and shall endeavor to maintain, the state of the lung parenchyma partakes more of the character of congestion than active inflammation. There can be but little doubt but that, in a great proportion of cases, *some degree* of inflammation exists, as proven by the presence of fixed pain—the peculiar sputa and other symptoms; but often, as we have premised, the expectoration is of a different character, partaking more of the appearance of diluted blood, or of a sero-sanguineous nature.* When, therefore, perfect dullness on percussion, and absence of the respiratory sounds, betoken condensation of the lung in this disease, it is not, in my opinion, due to an inflammatory process with lymph effusion, as in the frank form of pneumonia, and to which the term hepatization has been properly applied. We shall endeavor to substantiate these views when the nature of this affection is considered. Lungs, says Walshe,† which the naked eye would judge to be in the same condition in regard to their shares of air, fluid and semi-plastic substance, it is evident, acoustically, are in different physical states: specimens of parenchyma apparently identical are, in reality, widely different. In estimating the conducting power of lungs, there can be but little doubt that varying homogeneousness is a more important element than any so-called "solidification in pneumonia."

* Practical Treatise, Heart and Lungs, 131. † In this disease the expectoration is usually an exhalation, and not the result of inflammation.

"Dullness," says Blakiston,* "will result from serous or sanguineous congestion of the cellular portion of the lungs in contact with the walls of the thorax, whether of a passive character, being the result of obstruction to the circulation, or of an active character, as in the early stages of pneumonia."

Andral† considered hepatization to arise from congestion of blood, and not from lymph effusion, and in this opinion Stokes also concurs. Blakiston,‡ we think, very reasonably concludes, and cites experiments in proof, that in pneumonia there are two forms of solidification, one of which is produced by congestion, and the second by deposition of lymph.

Percussion Signs of Solidification.—"The thoracic walls," says Skoda, "beneath which the hepatized lung lies, yield a dull percussion sound, and their resistance is increased, provided the hepatized portion of the lung has the thickness of about an inch, and an extent greater than that of a pleximeter. The more extensive the hepatization, the duller is the sound, and the greater the resistance. If the sound is completely flat, as elicited in striking the thigh, then the lung is solidified throughout its whole thickness.

Auscultatory Signs.—According to Lænnec, in hepatization the crepitous rhonchus and vesicular murmur are absent in the affected part, and this is frequently the only proof afforded of solidification having occurred, bronchophony existing in certain cases if the inflammation is seated near the roots of the lungs, or in the superior lobes, where the bronchial tubes are largest. When the pneumonia is central, bronchophony is absent or obscure, becoming more manifest if the inflammation approaches the surface of the lungs; this being easily accounted for, he observes, as a hepatized lung is a better conductor of sound than a healthy one, bronchophony being nothing more than the resonance of the voice within the bronchi of the inflamed part. Bronchial respiration and bronchial cough, according to the same author, always accompany bronchophony; and when the hepatization is near the surface, and involves within it bronchial tubes of con-

* Practical Observations on Diseases of the Chest, 37.

† Pathological Anatomy, vol. i. 511. ‡ Diseases of Chest, 286.

siderable size, then it is frequently accompanied by a sensation of blowing into the ear ("*souffle dans l'oreille*"), and if a thin portion of pulmonary substance not yet hepatized intervenes between the ear and the affected bronchi, the sensation denominated the veiled puff ("*souffle voile*") is produced. The auscultatory sounds, as described by Skoda, more nearly accord with my limited experience. I, however, do not feel competent to pronounce on the correctness of his theory of consonance. He says: "Where the hepatized portion is extensive enough to contain within it at least one of the larger bronchial tubes, and if the tube contains air, is not obstructed, and communicates with the trachea, then the voice of the patient will consonate in the bronchial tube, and be heard either as loud or weak bronchophony over those parts nearest the tube." "The respiratory sounds vary according to the nature of the râles in the larynx, trachea or bronchi, through which the air must pass before it enters the bronchial tube surrounded by the hepatized tissue. The respiratory murmur may alone be heard, or, in addition to this, hissing, whistling and sonorous sounds, either separately or combined. If the hepatized portion of the lung be not large enough to contain one of the larger bronchial tubes, or if it be large enough, but the tube be filled with fluid or solid matters, or if the communication between the tube and trachea be obstructed by mucus, blood, etc., then neither bronchophony nor bronchial breathing, nor high, clear râles, nor hissing, whistling or sonorous sounds will be heard over the hepatized lung; the patient's voice will be *inaudible*, or recognizable only as a dull muttering, and the respiratory murmur *indistinct* or *absent*." Whether the explanation of Skoda is correct or not, I cannot say, but certainly, in a large majority of my cases, neither bronchophony, bronchial breathing, nor bronchial cough have been observed. I am inclined, however, to attribute their absence to the fact that, in this variety of pneumonia, the impermeability of the lung being due to excessive engorgement, and not to lymph exudation, it therefore does not possess the requisite density to make it a good conductor of sound.

THIRD STAGE—PURULENT INFILTRATION.

This is a very rare termination in this form of pneumonia. When present, the physical signs do not differ from those of solidification. A well-marked case of this condition came under my care, which was attended with copious purulent expectoration tinged with blood, being of a yellowish pink hue, the quantity evacuated being at least two pounds per day for several consecutive days. Laryngeal, tracheal and bronchial râles were present and distinctly heard at a distance of several feet from the patient. Percussion yielded a dull, flat sound over the whole thorax below the fourth rib, on each side, beneath which no sound of the voice, cough or respiration could be heard. Above this line percussion yielded a clear sound, and the ear discovered a coarse, humid rhonchus with large bubbles, evidently due, like the sounds in the windpipe above, to the passage of air through the abundant purulent secretion.

Signs of Resolution.—When this occurs, according to Lænnec, in the first stage, the crepitous rhonchus becomes daily less perceptible, and gradually yields to the normal respiratory murmur. When hepatization has taken place, its resolution is announced by the return of the crepitous rhonchus ("*râle crepitant de retour rhonchus, rhonchus crepitans redux*"). Skoda, on the other hand, maintains, and his views accord with my own observation, that the period of resolution is not invariably attended with the appearance of the crepitating râle, but in most cases is accompanied by a great variety of râles, or by whistling, hissing and sonorous sounds. In some rare cases, he adds, no râles whatever appear during resolution—the bronchial breathing being at first indeterminate, and at last vesicular. "The crepitating râle, or râle resembling it," he adds, "is heard during the resolution of moderately severe cases of pneumonia; it is also occasionally observed in severe cases, at a more advanced period of the resolution, when the secretion has become scanty. In the greater number of cases the vesicular breathing does not return immediately upon the resolution of the disease; but we generally find, after all the functions are restored to their healthy condi-

tion, and percussion no longer yields any abnormal sound, that the respiration still remains indeterminate, or that râles, or hissing, whistling, and sonorous sounds continue. Auscultation yields the same sound when the resolution is incomplete." In conclusion, this distinguished author thus sums up: "It follows, from the above, that the presence of pneumonia cannot be determined by the auscultatory signs alone; that these are often very indefinite, and that bronchophony, bronchial breathing and other sounds, as well as vesicular breathing and fine equal-bubbling râles, are signs which, of themselves, do not enable us to draw accurate conclusions as to the condition of the lung parenchyma. In forming our diagnosis we must also take into consideration every other symptom attainable by percussion, and by other means at our command."

DIAGNOSIS BETWEEN MALARIAL AND OTHER FORMS OF PNEUMONIA.

The varieties of pneumonia with which this affection is most liable to be confounded are, simple or idiopathic pneumonia and typhoid pneumonia. I shall therefore be necessarily compelled, in order to institute a comparison between these disorders, to glance at their predominant characteristics. I shall do this with all the brevity which the importance of the subject and my faculty of condensation will allow me to use.

Simple Pneumonia.—The celebrated Cullen, whose descriptions of the rational symptoms of disease were masterpieces of faithful delineation, and who doubtless drew his pictures from this form of pneumonia, says:* "Pneumonic inflammation, however various in its seat, seems to me to be always known and distinguished by the following symptoms: pyrexiæ, difficult breathing, cough and pain in some part of the thorax; and that, for the most part, the pulse is frequent, full, strong, hard and quick." This portrait of the affection continued to be the principal guide from the time in which he wrote until the occurrence of the modern discoveries in pathological anatomy and physical diagnosis. To the symptoms laid down by Cullen we have, therefore, the im-

* First Lines of the Practice of Physic, American edit., New York, 1793.

portant aids furnished by those valuable additions to our science. In drawing a comparison thus far between the general symptoms of these affections, we perceive at once a close resemblance, which would amount to identity, were it not for the dissimilarity in the character and variations of the pulse,—that in malarial pneumonia being frequent, soft, of moderate fullness, compressible, and never or rarely very full, tense or hard. Nor is the circulation in simple pneumonia subject to the same diurnal variations. It is true that writers aver that, in some cases of the latter form, the fever is sometimes evidently remittent; but, granting that these cases did not belong to the malarial diseases (though we strongly suspect the contrary), still I think that a marked paroxysmal character is seldom or never observed in the simple form. The regularly-recurring cold stage is another guide to discrimination, seldom absent in the malarial affection, whilst in the frank form the skin is uniformly of an elevated temperature in the great proportion of cases, generally being hot and dry at all periods of the day and night, whilst in the exacerbation of the former disease the heat is more moderate, and is subject to a daily decline in the remission which occurs immediately before the reäppearance of the chill. The evidences of hepatic and gastric disorders have gained for the malarial form the epithet of "bilious;" and although we utterly reject that view of its pathology which associates either this disease or any form of malarial disease necessarily with hepatic lesion, save as regards the occurrence of the latter as a secondary affection of minor importance, yet the frequent presence of nausea and bilious vomiting, in several cases, will doubtless afford some aid to diagnosis; those hepatic symptoms dependant on sympathy of contiguity, where the inferior lobe of the right lung is implicated, in pure pneumonia, being of exceptional occurrence. The condition of the bowels in this disease and the latter is also different. In the malarial form, the intestinal canal seems generally to be in a state of phlogosis or exalted sensibility. This is manifested by the frequent presence of diarrhœa; and when this is not evident, by the hypercatharsis often induced by the mildest aperients. Later, when the disease succumbs, convalescence is usually announced by those characteristic and, we may say,

critical alvine evacuations before described. The expectoration is often very different in the two diseases. In this it is usually less viscid, less copious and more fluid, partaking more of the character of blood mingled with serum; in fine, presenting more of the character of a defibrinized blood than as a product of inflammatory action. The presence of delirium is more common in the malarial than in the simple form. In the former it is usually distinctly intermittent or remittent in the early stages. At that period it also is usually mild in its character, the patient seeming to be in a state between sleep and awake, muttering incoherent and rapidly-changing fancies, but when fully aroused, and his attention directed, replying in a rational manner. The delirium is evidently of a neuralgic character, being often associated with a cool skin and feeble pulse, with no other symptom belonging to organic cerebral lesion, and, like the hallucinations of delirium tremens, to which it bears many and striking resemblances, is relieved by sleep, to recur again, however, in the febrile paroxysm. Delirium, however, supervenes under other circumstances, which, although we have already noticed, constitute such an important feature as will excuse a repetition. Often, without any previous cerebral symptoms whatever, after the pneumonic symptoms have been mitigated, or have entirely disappeared, and confident hopes are entertained of the patient's recovery, a furious delirium suddenly occurs. I can not refrain from adding this striking characteristic, as drawn by Cleghorn: "Besides some abatement of the fever, which happened every morning, it was remarked that, upon the third day, or beginning of the fourth, there was frequently a great remission, sometimes a total cessation of every violent symptom, so that the sick were thought to be out of danger; but, on the fourth or fifth, a delirium suddenly came on, or the breathing became more difficult than ever, and one or both of these symptoms increasing hourly, the patient expired, either suffocated or raving mad, unless, Nature or Art assisting, he had the good fortune to escape by some critical evacuation."* It has happened to the writer to have seen cases in which every *rational*

* Diseases of Minorca.

and *physical* sign of convalescence* had made their appearance, but who, on the next visit, were found to be in the same condition as when first attacked, without any known cause which could be possibly assigned for the change. In these cases it will be perceived that the malady was strictly intermittent, and the recurrence of the symptoms was evidently connected with the periodical character of the affection, and the liability of that class of diseases to relapse.

We have thus briefly pointed out the variations presented by the several organs, functions and secretions, to aid the beginner in discriminating between this and the simple form; but there is another phase of pneumonia with which it is also too often confounded, and which, of late years, is seen frequently even in malarial regions, and from which it is equally important that it should be distinguished, viz., typhoid pneumonia.

By the term typhoid pneumonia, I designate that species of pulmonic lesion which presents itself as a complication of the form of continued fever for many years widely prevalent in the Old and New World, and by general acceptation known as typhoid fever. It is true that this term and its synonymes have often been applied to simple and malarial pneumonia, but, I believe, on erroneous grounds. The designations typhoid, asthenic or sthenic, cannot be applied to the latter disease, as the exhibitions of vital force or reaction are too variable in their nature, intensity and duration to render them appropriate. Thus, as we have seen in some cases of the malarial disease, the vital powers were almost overwhelmed at its onset, continuing thus for days together, and at last, perhaps, followed by vehement reaction; whilst in the cases generally there were such diurnal changes that the same patient might have been regarded in an asthenic or sthenic on the same day, accordingly as he may have been seen in the remission, chill or exacerbation. It is true that the malarial affection, when continuing, finally places the patient in an adynamic state, but this is true of acute diseases generally, and must be regarded as a sequent and not as an original condition.

* See Synopsis of Termination in Health, p. 47.

The term typhoid can not be properly applied to any form of simple inflammation. In my opinion, pure inflammation of the lungs, occurring in a previously healthy subject, cannot present typhoid symptoms, unless it is associated with some morbid element which impairs the resistant powers of the organism. When, therefore, such a patient prominently presents the symptoms of pneumonia, attended with deficient vital reaction, the inflammation of the lungs is a secondary or collateral affection, and not the primary or essential malady under which he is laboring.

Typhoid pneumonia may be presented as an intercurrent affection, but often the inflammation of the lungs is the first and most conspicuous symptom. To such an extent and so marked is the expression of this lesion, that the practitioner is liable to regard the pulmonary affection as the sole cause of the illness of the patient, and not perceiving the system condition with which it is connected, may be led into the error of treating it as such. On minutely inspecting the case, however, he will be enabled generally to discern its true character, and recognize under the veil of pneumonia the peculiar form of fever on which it is engrafted. To the novice we offer a few observations as guides to a correct diagnosis. Typhoid pneumonia usually differs from the forms before mentioned—1st, In its mode of access. It is usually more gradual in its approach, and, in cases where the pneumonic symptoms announce themselves suddenly, it will be commonly found, on inquiry, that the patient has been indisposed for some time before their appearance: 2dly, In the character of the attendant fever. The regularly-recurring paroxysms of malarial pneumonia are absent; the firm, full pulse of the simple form is never observed; but, on the other hand, symptoms of great depression of the vital powers are generally present. In short, we are usually enabled to discover the peculiarity of the case by bearing in mind that the patient before him is the subject of typhoid fever with pneumonia superadded.* Lastly: In diagnosing the various forms of pneumonia, we should look around

* See the writer's article on Remittent Fever, for the diagnosis between that disease and Continued Fever, in the last Transactions; also, in Virginia Medical and Surgical Journal, January, 1855.

our patient and endeavor to ascertain the sources of disease to which he has been exposed, whether of transitions of temperature, moisture, contagion, endemic or epidemic influence. Although the rules laid down are generally applicable, we should ever be on the alert in fear of a mistake. It is desirable that it should be impressed on the mind of the beginner, that he will often be compelled to diagnose and treat many cases of which his knowledge must be chiefly drawn from the character of the epidemic present. Physical signs often interpret disease imperfectly; symptoms are merely outward expressions of an internal departure from normal vital action; they often vary, they are frequently obscure, and the same morbific agent has so many different modes of displaying its effects on the organism, that many instances must present themselves to the observer, in which no pathognomonic guides will be perceived, or, if present, undecidedly and imperfectly pronounced.

CAUSE AND NATURE OF MALARIAL PNEUMONIA.

The efficient cause of this disease, as its name implies, I believe to be malaria. By this term I desire to imply the identical cause of that class of pyrexiæ now generally known as periodical fever.

In employing this description, I do not commit myself to, or demand a belief in, any of the theories concerning its origin; but as nought save the great First Cause can exist without a creator, so periodical fever can not, without being called into action by some morbid agency. Whether that agent be solid, fluid, gaseous or imponderable,—whether it be

> "Airs from Heaven or blasts from Hell,"

or the

> "infection that the sun sucks up
> From bogs, fens, flats,"

is not desirable or necessary here to determine. Time was when my youthful mind delighted to sport with questions like these, and—

> "To float like Pyrrho on a sea of speculation,"

and I had the folly to believe, with numbers before and perhaps after me, that the problem was solved; but our days of dreams are over.

Nevertheless there are some axioms which may be regarded as established in reference to this source of disease, which, for the elucidation of my views, it is necessary to bear in mind.

1st. The primary impression of malaria is depressing in its character.

2d. Periodical fever may owe its existence to an exposure to its cause at a period considerably antecedent to its development.

3d. This class of fevers, though chiefly rife in the warmer, may occur primarily in any season of the year.

4th. A person exposed to malaria may present indubitable evidences of its pernicious influence, without any regular attack of fever having occurred, such as anorexia, nausea, malaise, diarrhœa, neuralgia, debility, palpitation, emaciation and anæmia.

5th. Persons constantly exposed to malarial influence may retain perfect health.

6th. In persons of this latter class, periodical fever is often suddenly developed by any depressing cause, such as a blow, a fall, a shock, and by the incidental occurrence of other diseases.

The doctrine that this form of pneumonia owes its existence to malarial influence has long been entertained and acted upon, but, on the other hand, has been as strenuously contested, both in regard to its correctness and legitimate consequences. The subject is one of great interest and of the highest importance, involving, unquestionably, the lives of the inhabitants of many sections of our own and other countries. The reader who has had frequent opportunities of observing remittent fever at the bedside, or those who have not, who have known the writer, by a perusal of his imperfect descriptions of that disease, can not fail to perceive the close resemblance between the phenomena presented by that affection, and the disease under consideration: indeed, the similitude is almost perfect, with the addition of the signs and symptoms of the thoracic lesion. It will be observed, therefore, that the evidences of organic implication are by no means confined to the organs of respiration; that symptoms of gastro-intestinal disorder very frequently preceded the lung symptoms, and that both were very often preceded by febrile excitement for hours, and even days. We have seen that the patient

evidently does not always perish from the pathological changes wrought in the lungs, death more often occurring from the supervention of fierce delirium, not only independent of the lung affection, but subsequent to its abatement or total disappearance. These facts, then, clearly denote the connection between this disease and periodical fever, as they sufficiently prove that its phenomena are not merely symptomatic of lesion of the lungs.

By some, an explanation of the existence of this disease has been given, by attributing it to the combined agency of malaria and atmospheric vicissitudes. This is doubtless often true, but it does not offer a satisfactory solution of the fact that many are often attacked with it, who have not been exposed in any appreciable degree to those changes. Indeed, this affection often attacks a number of persons consecutively in the same household, with intervals of time elapsing between the occurrence of the cases to permit all conditions of temperature and humidity to appear, properly belonging to the season. "It is probable," says Lænnec, "that the epidemic peripneumony is often owing to deleterious miasms which have entered the system by means of the cutaneous or pulmonary absorbents, since nothing is more common than to meet with cases of this disease to which we can assign no occasional cause. How many persons are seized with it in their very chambers, and in spite of the utmost care taken of their health."* It is impossible that any observer of ordinary perception can have failed to have been often impressed with this reflection. Exposure to atmospheric alternations does not, therefore, offer a complete and satisfactory explanation of the general occurrence of this affection. I propose, with some diffidence, to offer a few simple thoughts on the subject. It is a fact which will scarcely be questioned, that during the prevalence of any fever in seasons of low temperature, pulmonary inflammation is prone to occur as a complication. In scarlatina, rubeola, typhoid fever, and other febrile affections, it is very common. There are some causes, therefore, existing and belonging to the season, which either excite or predispose to lesion of the lung parenchyma. In

* Forbes' Translation, 233.

instituting an inquiry into the operation of those causes, it will be necessary to bear in mind that the system has passed through the fervid heat of summer, the balmy temperatures of autumn, by transitions more or less variable, to the colder season of the year. Usually the outward man has been prepared for this change, the whole surface of the skin having been protected from the effects of reduced temperature, by clothing of proper quantity and quality. But how is it with the lungs, the only remaining surface, besides its outlet, which is exposed to the direct contact of air? What has been done to protect that extensive, delicate lining of the air-tubes and air-cells; that admirably contrived membrane, abundantly supplied with blood-vessels of such remarkable construction and tenuity, as to allow the permeation of air through their delicate parieties, and yet allow, under ordinary circumstances, no escape of their contents? It is evident that this great annual change of temperature can not take place without some important physiological revolution in the pulmonary circulation, to understand the nature of which, it is only necessary to remember the usual effects of reduced temperature upon vascular tissues in general. The first impressions of cold upon the lungs must necessarily be succeeded by contraction of their capillary vessels. Happily, however, our organs are endowed with a certain degree of resistance to depressing causes, under the operation of which the vessels again expand and allow the free transmission of their contents. In short, the equilibrium of health is established. This resisting force which thus averts disease, or in other words, the nervous force, the only power existing in the system, of which we are cognizant, must, however, be continued and maintained, in order that no embarrassment to the circulation may ensue; consequently, throughout the colder seasons, a greater amount of nerve power must be expended in the lungs to preserve their physiological condition.

Again: The effects of the reduced temperature will be in proportion, 1st, to its intensity and duration; and, 2dly, the capability of resistance residing in the system.

If we suppose that the system is contaminated by the depressing influence of malaria, whether enervated by open attacks of

malarial fever, or debilitated by its latent influence, it necessarily follows that such a person must be in a favorable condition for the development of pulmonary engorgement, from the fact that he is subjected to a temperature requiring a healthy nervous force to resist, which he does not possess. There is plainly, then, a degree on the thermometric scale, equivalent to disease with him, yet compatible with health in others.

Let us advance one step further. The degree of cold being present beyond the nervous power to resist, or having endured so long as to have for a time exhausted its resources, let us look again at the capillaries of the lung. If the vessels contracted by the application of cold do not possess the normal resistant vigor, as we have assumed as an axiom in the malarial diathesis, the same or greater effect is produced than by a more intense degree of cold, acting on a healthy subject. Under ordinary circumstances, however, the nervous centres will be stimulated to react (those cases attended with collapse in the commencement being the exceptions), the reaction, according to a law of the organism, being in proportion to the previous degree of depression. "An increased afflux of blood to the part consequently ensues, and as the action of cold has indirectly produced the same effect as that produced by a direct irritating cause, it will only be necessary to follow the subsequent steps in the process to the state of active congestion, as detailed in Kaltenbrunner's microscopic experiments.*

But there is yet another function which has also to undergo an important modification. The skin, which in the warm seasons had been exalted to excessive activity, gradually falls into a state of comparative repose, as the bleaker season arrives. This evidently occurs, however perfectly it may have been protected. This subsidence of function must necessarily be followed by a determination of blood to the internal organs, the retrocession being greater and more easily induced in malarial subjects, for reasons before adduced. We have now an explanation of the modes by which cold and malarial influence may act as exciting

* Watson's Practice, Art. "Active Congestion," p. 46.

and predisposing causes of this affection; but this solution does not wholly apply to those cases occurring in previously healthy subjects, whilst sedulously guarded from atmospheric vicissitudes. These cases are evidently due to the evolution of malaria, the affection of the lungs being produced by the predisposing tendencies of the season. Objection has been urged on many grounds to this view of its causation, the principal one of which is that pneumonia prevails in seasons inimical to the existence of this mysterious agent. This is plainly an error. Primary attacks of periodical fever not unfrequently occur during the winter and spring months, especially in the latter, which is also the season in which this form of pneumonia is usually prevalent. The opponents of the views I have embraced seem to ignore this established fact, as old as the ancient couplet—

"An ague in spring,
Is fit for a king." *

They have, however, been compelled to yield the admission, that a form of disease similar to that we have described often prevails as an epidemic. Now, whether it be regarded merely as a complication, a compound, or as a hybrid affection, it can not be disputed that it rages in those seasons of the year, the temperature of which they assume to be destructive to malarial influence. The inconsistency is obvious.

The occurrence of periodical fever, not only in the spring, but occasionally in the winter months, being easily established,† there are conditions necessary to notice, closely associated with its nature and phenomena, which, besides the tendencies of the season, assist in producing a sanguineous determination to the lungs. When discussing the subject of remittent fever, I briefly endeavored to prove its nature from the character of the symptoms existing during life, and to show that there was constant tendency in the vital current to retrocede from the extremities to the trunk, and in grave cases, from the whole surface to the internal organs.

* Elliotson's Practice, 242.

† Under the name of "adynamic remittent," I have described a form of remittent fever often seen in the winter season in my region. See article before referred to.

Of the cause of that morbid afflux, I then avoided discussion. I do so now, but in my opinion it is produced by an irritation of the nervous centres, of a peculiar character. As a consequence of this irritation and congestion, delirium, difficult locomotion, independent of pulmonary engorgement, with varying conditions of pulse, and abdominal symptoms, were observed, accordingly as different portions of the cerebro-spinal axis may have been involved.

To sum up our reflections on the pathology and etiology of this form of pneumonia, we have therefore to explain its production and phenomena.

1st. The predisposing tendencies of low temperatures to produce an afflux to the lungs by direct contact with their interior surface.

2d. The same cause, by repression of the cutaneous evacuation, determining to the whole internal organs.

3d. The operation of malarial influence, which not only diminishes the vital resistance to cold, but possesses an evident inherent tendency to produce internal engorgement.* A glance at the pathological condition of the lung will close our observations on this division of the subject.

I have repeatedly stated, in the foregoing pages, my impressions in regard to the condition of the lung parenchyma in this affection, as partaking rather of the nature of active congestion than well-developed inflammation. This is proven, in my opinion, by the following circumstances: 1st. The sudden occurrence of solidification, as evinced by perfect flatness on percussion, and absence of the respiratory murmur. This sometimes takes place, in grave cases, in a few hours after its onset, involving more than half of the entire lungs, accompanied by a cold skin, flagging

* Broussais, who doubtless saw a vast number of cases of this disease whilst serving in the French armies, and who has noticed at great length the connection between malarial fever and pneumonia, was fully impressed with similar views: "Cette concentration des forces à l'intérieur, ou si l'oncondamne ces expressions, cette accumulation violente du sang dans les capillaires des viscères, qui existe durant la periode de froid des intermittentes, devient surtout funeste au poumon pendant la soison froide." (Histoire des Phlegmasies ou Inflammation Chroniques, tome ii. 588). See, also, the works of Lænnec (op. cit.), M. Bailly (Traité des Fièvres Int., Paris, 1825), and Cleghorn (op. cit.).

pulse and colliquative diarrhœa, a condition of system totally incompatible with the existence of the inflammatory process. 2dly. The disappearance of the physical signs is too rapid, in many cases, to suppose that inflammation, terminating in plastic exudation, had produced this impermeable condition of the lungs.

It is true, as we have stated, that there are some evidences of inflammation* mingled with the symptoms, and as active congestion is a state closely allied to inflammation, the latter may, and doubtless does occur, but it is evident that inflammation can not be the general condition, as it is too expeditiously removed, in many cases of several days' duration, and by inadequate means, to render that view admissible.

TREATMENT.

In the Chill.—In this stage it is evident that the state of internal congestion is most intense; the indications are therefore to remove or lessen this condition. As, in ordinary cases, the vital powers are competent to produce reaction, the employment of active measures will be unnecessary, but when the cold stage is serious and protracted, the usual means adopted in remittent fever should be called into requisition. "Warm applications are to be assiduously made over the whole length of the spinal column and to the extremities, together with stimulating frictions," and to these may be added warm cataplasms of mustard or capsicum to the anterior and lateral surfaces of the thorax. If these means should be unsuccessful in restoring the circulation to the surface, enemata of the sulphate of quinine may be used; 8 or 10 grains of the salt dissolved in 2 ounces of mucilaginous fluid may be thrown into the rectum every half hour, or more often, until symptoms of reaction ensue. If diarrhœa has been or is present, the following enema will be more efficacious:

Quinæ, sulph. . . . . grs. x;
Morphiæ, sulph., gr. $\frac{1}{8}$;
Acid. sulph. dilut., gtt. x;
Aquæ . . . . . oz. ii.—M.

To be used as the former, but at longer intervals.

* In Lænnec's cases, he only mentions "that *traces* of inflammation" were found after death. (Op. cit., 249).

The action of the enemata may be assisted by the administration of carbonate of ammonia by the mouth, or infusions of ginger or capsicum, whilst warm, may be given. In extreme cases it will be necessary to resort to alcoholic stimuli. These, however, should be used with caution, and their effects closely observed.

Fortunately, however, it is rarely that much difficulty is encountered in conducting the patient through the cold stage, and medication may be generally postponed until the febrile reaction is developed.

The Exacerbation.—The indications in this stage are to calm the action of the heart, to remove congestion, and to prevent the occurrence of the next paroxysm. If the pulse is hard, full or tense, as it is in rare and exceptional cases; or if the patient is robust or previously healthy and possessing ordinary vigor, and the pain or dyspnœa is very intense, and the character of the pulse or other symptoms do not decidedly contra-indicate its employment, then a moderate quantity of blood may be taken from the arm. In this disease general blood-letting should be employed with a view *only* to *moderate*, and not with an expectation to cut short the disease, nor should it be employed except in the instances referred to. In cases where this is not followed by marked relief, and in those where venesection is inadmissible, the local abstraction of blood by cups or leeches should be resorted to, and may be used as freely as considerations of safety will permit. The time at which bleeding,* by any mode, should be practised, is that period when the exacerbation has reached its acme, which is almost invariably in the afternoon or evening.

The administration of medicine is generally postponed until 9 or 10 o'clock P.M., at which time a full dose of calomel (15 or 20 grs.) is administered. If diarrhœa, or a tendency thereto, is

* "In the remittent fevers denominated peripneumonic," says Lænnec, "it may, no doubt, be sometimes necessary to bleed during a paroxysm, in order to prevent suffocation, but the utmost caution is requisite not to destroy unnecessarily the strength of the patient. We must ever keep in mind that blood-letting can not cure a disease which will certainly return after a few hours with fresh violence, and of which experience has long since demonstrated bark to be the only remedy." (Op. cit., 253).

present, 8 or 10 grains of Dover's powder are combined with the mercurial. Two or three hours thereafter this is followed by a full dose of quinine (15 to 20 grs.) in pills, or diffused in a wine-glassfull of cold water. Three or four hours are now allowed to elapse; we now usually repeat the quinine in doses of 5 to 10 grains, in proportion as the pulse may have been reduced, until 35 or 40 grains have been administered, and the patient is then left at rest.

According to my experience, the chill cannot recur whilst the patient is under the influence of the above quantity of the alkaloid; the fever may, however, continue, but usually in a very moderate degree. Should this be the case, the local bleeding is repeated in the evening exacerbation, and calomel, in small doses of 1 or 2 grs., with ½ to 1 grain of ipecac., or 2 grains of Dover's powder, is given every one or two hours. After midnight the quinine is repeated in the same manner, or in lesser quantity (in direct proportion to the degree of fever present), until the period of chill has passed, when all medicine is again suspended. It is rare, indeed, that cases thus treated in the early stages do not exhibit the most flattering amendment on the day succeeding the first night on which this plan is adopted; and still more rare, that every dangerous symptom has not disappeared on the succeeding day. Should the pneumonic symptoms, however, persist, the mercurial should be continued until its constitutional effects are produced. In some cases, where the arterial action is not disposed to yield, and the dyspnœa continues, I have ventured on the use of tartar emetic in small doses, in union with quinine. I am indebted to Dr. Blakiston for a knowledge of the value of this combination. The antimonial should, however, be carefully used; and it is better to commence with a minimum dose of 1-10th or 1-12th of a grain, to be gradually increased as it may be borne. I think it proper to say, too, that the gastric irritation and tendency to diarrhœa, so often present in this disease, contra-indicate the employment of tartar emetic, generally. Whether alone or in combination, the quinine should, however, be continued for several days after the active symptoms have disappeared. Blisters may be employed at almost any stage of

the affection, but under this mode of treatment may often be altogether dispensed with. Of course their use is preferable after the arterial action is dispensed with.

We could say more, but we have already, we fear, outwritten the patience of the reader: that which we would add would, however, be chiefly an extension of our views in regard to the use and action of quinine. But mark the prophecy! However much may have been written, the category of diseases has only been commenced, which will yet yield to its sway, whose destiny is not only to remove, but to unveil, perhaps, the essence of morbid action itself. Mysterious, wondrous, potent gift to humanity, more precious than the gold or gems of the sunny land beyond the burning zone, decked by thy imperial flowers! Minister and missionary of Nature! Science places an illuminated crown upon thy snow-like brow, and proclaims thee sovereign Queen of the vast medical realm! Of old, the Magian would have bent in adoration before thee, whose powers, surpassing the results of reason and the inspirations of genius, eclipse in certainty, celerity and brilliancy the fabled incantations of magic!

OTIS FREDERICK MANSON.

GRANVILLE COUNTY, N. C., April, 1857.

CARCINOMA UTERI.—(By Dr. Skinner.)—The two best deodorising agents in carcinoma uteri are Condy's fluid and tar-water, both, of course, used as injections. Of Condy's fluid, an ounce should be taken to a pint of water, and a cupfull used as injection thrice daily. For preparing tar-water, the following is the best formula:—Tar, lb. j.; water, conj. j.: mix, stirring with a stick for a quarter of an hour. As soon as the tar has subsided, strain the liquor, and keep it in well-closed jars. Higginson's syringe is invaluable in these cases. A tepid solution of soap and water should be injected first, and immediately afterwards a breakfast-cupfull or more of undiluted tar-water. The syringe should have a long vaginal nozzle, that the injection may come in contact with the ulcerated surface.—*Braithw. Retrospect.*

LITHIA.—The carbonate of lithia is highly extolled in gout. It forms, when ingested, the most soluble salt of uric acid known.

Physiology.

THE ANATOMY AND PHYSIOLOGY OF THE PLACENTA—THE CONNECTION OF THE NERVOUS CENTRES OF ANIMAL AND ORGANIC LIFE.

BY JOHN O'REILLY, M. D.,

Licentiate and Fellow of the Royal College of Surgeons, Ireland; Resident Fellow of the New York Academy of Medicine; Member of the Medico-Chirurgical College of New York; formerly Medical Officer to the Oldcastle Workhouse Infirmary and Fever Hospital.

PREFACE.—No one is more fully sensible of the difficult task of unraveling the mysteries connected with the nervous system than I am. No person is more firmly impressed with the conviction that it requires a man of profundity of thought, depth of penetration, expansive intellect and enlarged views, to illustrate and demonstrate the mode of action of the nervous system, than I am. No person is more forcibly convinced of his incompetency to give a subject of such extraordinary importance ample justice, than I am.

What, therefore, has induced me to attempt writing on a subject which, during a period of twenty years, whilst actively engaged in the practice of Medicine and Surgery, I never bestowed a thought on?

The reply is, simply, that, being accidentally called by the Section of Anatomy and Physiology of the Academy of Medicine to attend one of its meetings, with the view of forming a quorum, to enable that body to proceed with a discussion on the Anatomy and Physiology of the Placenta, I complied with the request, and casually advanced views which I subsequently found necessary to vindicate, and which I am now anxious to place before the profession *in extenso*.

Introduction.—It should be of no consequence by whom the true solution of an abstruse question is found, provided it can be proved to be correct.

Every person who enters on an inquiry relative to a difficult subject, with a view to expound it clearly and satisfactorily, is looked upon with suspicion; his assertions regarded with doubt; and whatever he puts forward as fact is received with hesitation.

The high reputation of an author is generally a passport to

the public that the subject he treats of is worthy of attention, and demands consideration.

Conclusions and opinions, arrived at by men after superficial reading and study, are not calculated, in many instances, to elevate the reputation of an author, as they cannot appreciate or comprehend the nature of the matter under discussion.

The opinions of the highest authorities on a scientific subject should not be received as authentic, without standing the ordeal of the most rigid scrutiny as to their truthfulness in all its bearings.

It is particularly requested that the reader of the following pages will negative or affirm every paragraph, after mature consideration, without waiting to read the entire chapter.

The great obstacle to the thorough understanding of the nervous system of animal and organic life presents itself in the want of human intelligence of a sufficiently high standard to comprehend the agency of immaterialism in the operations of materiality.

The several organs of the body are constructed physically with the greatest precision as to their mechanism and component parts, so as to discharge efficiently the duties of the offices which they were destined to perform in the animal economy.

The eye can not be surpassed in construction, as an optical instrument; the ear, as an acoustic apparatus; or the larynx, as a musical instrument.

It is to be observed, when the eye is injured, vision is either lost or impaired; that the same remark is true of hearing, when the ear is implicated; that the voice is lowered when the larynx suffers; and so, in like manner, when any part of the organic nervous system is treated with violence, the functions of life are deranged, or cease altogether.

The anatomical organization of a dead man is the very type of the living one. Certain characteristics, which are too well known to require enumeration, distinguish the living from the dead; as the former can be instantly assimilated to the latter, without any trace of disorganization being perceptible; it follows thence, as a sequence, that an immaterial or imperceptible agent must have been expelled.

The union, therefore, of an immaterial agent with materiality, scientifically arranged, as is exemplified in the case of the organic nervous system in man, constitutes life.

The severance of the immaterial agent from the material substance, resulting in death, requires elucidation.

The immaterial agent, known as life, is held in existence by the union of the oxygen of the blood with the structure of the organic nervous system. Whatever, therefore, disturbs this process is followed by fatal consequences; as, for instance, a violent blow on the semilunar, the superior cervical, the cardiac, or the central cerebral ganglia; the shock given to one ganglion is instantly communicated to all the others; thereby the animal machinery suffers concussion, and is unable to attract or combine with the oxygen from the blood: thus the immaterial agent, whose existence depends on the harmonious action of these material agents (to wit, the organic nervous system and the oxygen), disappears from its abode.

A dead man and one in a sound sleep closely resemble each other as regards communication with the external world; the operations of the immaterial agent, for which the encephalon was established to provide a habitation for, being totally suspended. The distinction between a dead man and one asleep consists in the operations of the immaterial agent in the organic nervous system being carried on in the latter, whilst in the former they are not.

These facts demonstrate the distinction of the action and duties of the immaterial agents in the animal and organic nervous system.

A dead man—one recently executed—can be made to appear, to a certain degree, like a living one, by substituting an immaterial agent for the one that has departed—namely, galvanism. It is well known that one wire of a galvanic battery applied to the lumbar nerves of a frog, whilst the other touches the foot, will cause contraction of the muscles.

It is highly important to understand that the Creator has invariably used material substances when carrying into execution immaterial agencies; thus, for instance, man was made of the slime of the earth: it is evident that without Omnipotent

power this could not be accomplished. When Moses struck the rock of Horeb with his rod, water came forth: it is equally clear that an all-powerful, invisible agency was in operation.

The most extraordinary instance of the power of immaterialism being combined with materiality is exemplified in the Incarnation. I cannot avoid giving the text from the first chapter of St. Luke, verse 25: "The Holy Ghost shall come upon thee, and the power of the Most High shall overshadow thee. And therefore the Holy Ghost which shall be born of thee, shall be called the Son of God."

A great many look upon the Bible as fabulous, while numbers can not comprehend its sublimity, or construe it to meet their own limited or presumptuous views; in a word, ridicule the idea contained in the above quotation from the Gospel, and thus ignore altogether the power of immaterialism over materiality. There are others who endeavor to account for the phenomena of life by close anatomical investigations of the minute structure of the body, or by chemical agency; who disregard immaterialism, as unworthy of scientific research, and hold it derogatory to their reputation for keen examination and learned exposition.

It is necessary, therefore, to remind the classes of persons alluded to, that the power of an immaterial agent over a material one admits of farther illustration. A bar of steel or iron, which has been magnetized, either by the application of a loadstone or a current of electricity, will attract another bar of iron and communicate a similar power to it. If two bars of iron are magnetized, the north pole of the one will repel the north pole of the other, whilst the north pole of one will attract the south pole of the other.

Could a clearer demonstration be given of the power of immaterialism over materiality? Does not one bar of iron, made magnetic, impregnate another bar of iron? Is not the analogy of impregnation still further borne out by recollecting that a piece of iron rendered magnetic is capable of attracting to itself other particles or scales of iron in its vicinity for a considerable time—thus enlarging itself precisely as the immaterial agent in the fœtus appropriates to itself the nourishment it requires from the mother?

Electricity is an immaterial agent: it can be generated from matter, as is witnessed in the case of the galvanic battery; and can be conducted thousands of miles in a second, through wires of various kinds of metal, and the phenomena of electricity rendered manifest by the electric spark.

The current of electricity, when the positive and negative wires of a galvanic battery are brought in close contact, will convert gold into fumes, or brilliantly ignite charcoal.

The power of the immaterial agent is here apparent; and it must strike the most superficial thinker that the wires are the mere conductors or instruments — a matter susceptible of proof —shown by cutting off the communication between the wires and the battery.

If the wires connected with the galvanic battery are put into a mixture containing gold, arsenic, copper, iodine and zinc, the metals will be found reduced to their metalline state by the action of the wires. No person will, I presume, attribute this agency to the wires, but must ascribe it to the immaterial agent they conduct or give abode to.

I trust it does not require further argument to demonstrate the mode of action of the immaterial agent, known as electricity, when in combination with a material substance — such as steel, iron or copper.

If an inanimate immaterial agent, such as electricity must be admitted to be, is capable of producing such extraordinary phenomena, what must a living immaterial agent be capable of accomplishing?

In the former case, the agent is brought into existence by an electrical machine, or galvanic battery; in the other it is not generated, but held in existence through the instrumentality of the union of the oxygen of the blood with the organic matter in the organic nervous system — the nerves acting as conductors of the vital agent as the wires of electricity. In the case of the galvanic battery, the acid plays a conspicuous part by its action on the zinc in generating the electricity; in like manner, the oxygen discharges a prominent part in its action with the tissue of the organic nervous system, in holding the immaterial agent known as Life.

It is exceedingly difficult to understand, and much more to explain, the manner in which the animal immaterial agent is obtained, as well as rendered active, in the encephalon. The operations of the mind are truly immaterial, and can be transmitted to distant parts by nervous bands acting as conductors, as is exemplified when the heart jumps in the pericardium on the receipt of exciting intelligence, or by an act of volition, as is witnessed when a person wishes to flex or extend his toes—the immaterial agent generated in the brain being thus conducted to the part specified.

The cerebrum, as well as the cerebellum, is composed of gray-and-white matter, folded up in a peculiar and compact manner. If the brain were fully unfolded, it would be found having a layer of gray substance on the outside and white on the inside.

The structure of the brain contains a large amount of phosphorus combined with oleaginous matter, and is thus rendered susceptible of excitation.

It would seem that the folding up of the brain was done for the purpose of affording a large surface or space for the action of the parts required to be carried into operation in producing or rendering active the immaterial agent.

Arguing from analogy, it would appear, in fact, that the arrangement of the gray and white substances is founded on the same principle that exists when a large quantity of electricity is required to be produced—namely, placing two large sheets of zinc and copper over one another, separated by horse-hair, and then coiled up; thus occupying a small space, and easily brought into action by immersion in an acid mixture. So, in like manner, the gray and white substances are brought into operation through the agency of the phosphorus.

This explanation is an hypothesis, but appears to be borne out to a certain extent by facts. When a man overworks his brain, phosphorus is found in the urine; showing it has been used to excess in mental operation. The brain requires rest from excitation; hence sleep is called for to suspend its operations, and allow the process of recuperation to take place.

Certain parts of the brain are destined for specific purposes,

as can be deduced from the connection of the cerebral nerves. The immaterial agent allied with the sense of smell is in one place—vision in another—hearing in a third—taste in a fourth, and so on with respect of all the functions of the brain. Each has its own place assigned it.

Thus it is certain parts of the brain preside over particular functions allied to animal life, just as the organic ganglia preside over the functions of the organs with which they are connected, each having a special duty assigned it.

It is a remarkable fact, that whereas the gray substance is placed on the external surface of the convolutions of the brain, that in the spinal cord it is located internally. Equally worthy of notice is it, that the current of the immaterial agent in the brain is from without inward towards the centre or mesial line, whilst in the spinal cord it is from within outward. In both cases the arrangement is in accordance with the connection of the nerves with the brain and spinal cord.

The conclusion from these circumstances would be evidence that the phosphorus acts on the gray substance, so as to produce the immaterial agent, in a similar way that the oxygen unites with the tissue of the organic nervous system in the sustenance of the immaterial agent known as Life.

It would seem that the white fibrous structure of the brain acts as conductors, as also repositories, of the immaterial agent after being generated. It requires a certain time to study a subject—the mind, while doing so, is immaterial. The result of the operations of the mind is immaterial, and is deposited in the white substance of the brain, to remain there until called for, in some measure similar to the manner in which electricity is procured in the Leyden jar.

It is a very perplexing matter to conceive how the iris contracts and dilates under the influence of the lenticular ganglion—the heart under the cardiac ganglion—the uterus under the uterine ganglion, or the arteries under the organic nerves, derived from the organic ganglia.

In truth, it would appear preposterous to some persons even to contemplate that such an extraordinary result should take place through their instrumentality.

But analogy will quickly reflect light on this mysterious subject. For instance, if two solid bars of iron, on being magnetized, repel one another. the positive poles being placed in juxtaposition, will not dilatation be the result? If the south of one is attracted by the north pole of the other, will not contraction be the result? Hence dilatation and contraction are observed alternately taking place between the two bars. The bars contain an immaterial agent, known as electricity; the nerves an immaterial agent, called Life. Hence the phenomena in question can be understood.

When a person sees disgusting food, the stomach rejects its contents. How so? The organic nerves act much in the same way, under such circumstances, that the positive pole of one magnet does when it repels the corresponding extremity of the other.

When two wires of a galvanic battery touch one another at their extremities, a current of electricity takes place powerful enough to convert gold into fumes or brilliantly ignite charcoal. When food, after the process of mastication, gets into the stomach, the gastric juice is secreted by the stomachic nerves, under the influence of the stomachic ganglion, and is capable of dissolving the hardest substances.

The lacteals, under the influence of the mesenteric ganglion, absorb the nutritious portion of the chyle, just as the wires of a galvanic battery will collect gold or silver in a metallic state in a mixture, where the metals are united with compound bodies.

The phenomena just detailed cannot be attributed to the wires in the one case, nor to the nerves in the other, but to the immaterial agents they conduct. A wire which will convey a current of electricity thousands of miles in a second is many thousand times smaller in proportion to the distance it has to convey the immaterial agent than the smallest nerve in the body. When a wire is cut across. the electric current is interrupted; divide a nerve, and the immaterial agent is arrested in its progress. A person wishes to flex his index finger; what takes place? The immaterial agent is generated in the brain, passes through the median nerve to the finger. Sever the nerve at the palm of the hand, and the message is interrupted. Bad news conveyed to

the mind is sent to the heart, and sometimes interferes with the good results anxiously expected by a surgeon after a capital operation—as every practical surgeon is made cognizant of by the intermission of the pulse; whilst good news is not only communicated to the heart, but to the fœtus *in utero*, by a continued chain of nervous communication: "For behold, as soon as the voice of thy salutation sounded in my ears, the infant in my womb leaped for joy." (St. Luke, iv. 44.) Here is a good example of immaterialism acting on materiality. Hence, direct telegraphic communication may be said to exist between mother and child.

I will now conclude by remarking, that the manner in which the arterial blood is converted into venous is by the absorption or union of the oxygen with the ganglia in which the capillary arteries terminate and the capillary veins commence.

The retinæ of organic nerves on the external surface of the arteries form ganglia at their extremities, through which the blood passing, loses its oxygen, and consequently becomes venous. It may not be amiss to remark, that *no organic* nerves can be found in connection with the veins.

Hence the difference between the actions of veins and arteries.

Surgery.

ON SOME COMPLICATED INJURIES ABOUT THE SHOULDER JOINT.

By Frederick C. Skey, Esq., Surgeon to St. Bartholomew's Hospital.

The following remarks are abstracts from a clinical lecture delivered at the Hospital by Mr. Skey. He commences with the most frequent form of injury to which the shoulder is liable, and proceeds subsequently to other injuries which, though not of infrequent occurrence, he thinks occasionally escape detection.

If you inquire into the nature of the violence which has led to dislocation of the *humerus* (not of the *shoulder*, as it is commonly termed, for the shoulder, correctly speaking, is incapable of dislocation), you will generally make out that in the act of falling the man's arm has been extended for the purpose of break-

ing the fall, and that direct force has been applied on the under surface of the limb, whether hand fore or upper arm; the result of which is, that the head of the humerus is thrust out of the glenoid cavity, by a counter force rupturing the lower part of the fibrous capsule. The ultimate position of the head of the bone depends much on that of the arm at the moment of the fall; whether extended straight from the trunk—the most common position, when the head falls into the axilla,—or forwards, or backwards, when it is found in either case in the opposite direction.

I shall, however, take as an example a case of dislocation into the axilla. The shoulder is flattened by the removal of the support, as it is termed, given by the head of the bone to the deltoid, which, being stretched, shows the clean outline of its origin. If you pinch it, you find it firm and tight—so tight, indeed, as to draw the elbow away to the extent of about four inches from the side of the body. If you attempt to approximate the arm to the trunk, the latter will recede, because the movement is a painful one, by drawing yet more positively on the already stretched deltoid and supra-spinatus muscles. If, on the contrary, the arm be raised, and the deltoid relaxed, pressure of the fingers upon it will detect the absence of the normal support it possesses in health, and the head of the bone will be felt in the axilla. If the hand be now applied in a similar manner to the opposite axilla, the different relative position of the bones will be obvious; and, finally, it will be found on measurement that the affected arm is about an inch longer than the sound one. And this sign alone is conclusive as to the existence of dislocation, because the scapula being unchanged in its position, if the humerus extends an inch longer than the opposite bone, it is clear that its head can not reach the socket. Add to the above the acknowledgment of a recent injury, and you have abundant evidence of simple dislocation of the humerus into the axilla.

The only variety of this accident to which I shall refer is that of dislocation forwards and inwards, in which the head of the bone is carried far upwards towards the clavicle, and when it is said to lie under the pectoralis minor or under the coracoid process, and which process obstructs its further progress in that

direction. This is a rare variety of this dislocation, and I should not have alluded to it but for the purpose of marking two features peculiar to it—1st, that the arm is rather shortened than lengthened; and, 2d, that it is usually accompanied by numbness of the arm, consequent on pressure of the head of the bone on the brachial plexus. The numbness accompanying ordinary dislocation is either absent or very partial; in this variety it is large and general. In both instances it subsides on the reduction of the displaced bone. I remember, some thirty-five years ago, a man being brought into the hospital with a dislocated humerus. While undergoing the necessary manipulation, distinct crepitus was felt in the joint. Its nature and locality were uncertain. The injury was very deliberately examined by the entire surgical staff. Much discussion arose on the case, which was evidently one of dislocation. However, the bone was reduced by the ordinary method adopted at that era,—namely, by extension of the arm outwards,—and the crepitus was felt no more. The man recovered in the usual time. A second case presented itself within the year, the entire features of which were precisely similar to the former one.

The impression produced on the minds of many observers of these cases has ever been, that it is possible to have, as the accompaniment of dislocation of the humerus, a crepitus, not caused by a fractured bone, but arising from some unusual injury sustained by the joint; and for many years I was myself inclined to adopt the same explanation, but later experience has taught me otherwise. It appears to me more reasonable to infer in these cases an undetected fracture, than the presence of crepitus without fractured bone. I cannot comprehend the sounds or sensations of crepitus to be caused by the attrition or rubbing together of any two structures of which the shoulder-joint is composed; and under these circumstances I cannot doubt that the injury in both these cases consisted of fracture of the glenoid cavity superadded to dislocation; and when it is recollected that the mode of reduction adopted at that period consisted in drawing the humerus outwards, it is not surprising that the fracture should escape detection during the operation; and if it then escape observation,

it is not probable that it would attract notice afterwards, because the simple agency of rest would be available to the recovery from both the fracture and the dislocation.

While employing the term "crepitus," I may observe that there are three distinct varieties of sensation to which the term may be applied, and which it would be well for you to observe. The first is the sensation conveyed by the rubbing together of the two fractured ends of the shaft of a long bone in a healthy subject; the second, the sensation peculiar to the rotation of the thigh in fractured neck of the bone in very old or debilitated persons, which resembles the cracking of an egg-shell; the third, the sensation occasionally conveyed to the touch by the movements of a diseased joint. The last of these is very distinct from the other two, and is the product of tissues less hard than bone. The term "crepitation" is limited to the breaking down of membranous tissues by the pressure of air confined within them. In the two cases I have above quoted the crepitus was clear and distinct, and, as I consider, could have been obtained only by the attrition of a broken bone.

The form of injury which I am about to describe is that of fracture of the glenoid cavity. A man sustains a fall on the shoulder, with the arm parallel to the body, but the force of the blow, whether the result of a fall or of direct violence, is thrown *on the deltoid*. The result is the fracture of some bone, indicated by palpable crepitus. Observation will teach you to judge tolerably correctly, by the sensation thus obtained, whether the crepitus indicates a large or a small fracture, and such knowledge will prepare you for a correct diagnosis of the real injury. Now, the diagnosis of fracture of the glenoid cavity is obtained chiefly by negative evidence, and in recalling to your recollection the bony structures immediately around, you subject each to a careful examination. You examine the clavicle, acromion, and the spine and coracoid process of the scapula. You find all bear forcible pressure, and may therefore be inferred to be sound; you next examine the head of the bone. Pressing your hand firmly on the deltoid, you rotate the arm freely, and you find the head to move as freely with the shaft, and the movement to be un-

attended with pain. You repeat this examination by raising the arm at right angles with the body, and pressing the thumb forcibly on the head of the humerus in the axilla, you again rotate, and the movement is both painless and inaudible, revealing no fracture. Nothing remains but the scapula, which, if you are correct in your diagnosis of crepitus, must be broken. It can be nothing else. If you now, while pressing the head of the humerus against the glenoid cavity, rotate the bone, the mystery will be solved, and crepitus will be evidently the result of fracture of a greater or less portion of the glenoid cavity. This evidence can only be obtained in one way, viz., by pressure of the bone against the broken socket. The existence of fracture is borne out by the direction as well as the violence of the force which has caused it, and by the presence of local pain, due not to the broken bone merely, but to a corresponding injury done to the soft parts over and around it. If you have arrived at your conclusions hesitatingly, if a doubt remain on your mind as to the existence of fracture, the patient himself will often solve it on putting the question to him, Have you broken a bone?—and especially if a man of somewhat more than ordinary intelligence; because, however palpable may be that compound sensation of hearing and touch we term crepitus, it is still more palpable to himself when his mind has been directed to it by the probably protracted discussion that has taken place on the nature of his accident. When a patient is made cognizant of the nature of the accident, his evidence becomes valuable. These cases cannot be readily mistaken for dislocation, for in truth they have no sign in common with it, except the disinclination to move the arm, which is universal in its application to all injuries in this region; but especially is there no diminution in the rotundity of the shoulder, and no increase in the length of the arm.

Several of these examples of fracture of the glenoid cavity have come under my observation during the last year. Both the nature and the extent of the injury are readily detectable by the inductive method of examination I have described. The extent of the fracture is not always so easily determined; yet by carefully observing the character and amount of the crepitus, and

noting whether it is readily produced by the act of elevating the arm, and pushing the head of the bone against the socket, we shall make a close approximation to the truth.

Fracture of the glenoid cavity may be complicated with dislocation of the humerus; but, so far as my own observation leads me, I believe it to be far more uncommon than simple fracture without dislocation. If the fracture, as usually occurs when superadded to dislocation, is confined to a small portion of the glenoid cavity, the signs of fracture merge in the apparently larger accident of dislocation, the evidence of which is uninfluenced by the presence of the injury to the bone. The shoulder is flattened, the arm is lengthened, the head of the bone revolves on the thumb pressed upon it in gentle rotation of the bone, while the fractured scapula lies behind the head, and escapes observation, unless the violence has been unusually great and the fracture unusually large, and comprising one-half or more of the socket from which the head is thrown. The evidence of dislocation is conclusive, while that of fracture is so imperfect and is so uncommon, that the injury escapes observation. The heel is applied, the bone is reduced, and although the return of the head into the socket may be accompanied with some unusual sensation, which is really that of crepitus, the reduction has been so satisfactory, the restoration of parts so complete, that the question of fracture may scarcely enter the thoughts of the surgeon. Nor does there arise in the after-history of the case any circumstance by which to determine the occurrence of fracture. The fragments are brought into contact by the act of reduction, and in the course of a few weeks are united.

A case of more than usual interest occurred to me lately in the person of a man, aged sixty, in Abernethy ward, who had sustained an injury to the shoulder three weeks prior to his admission. The head of the humerus was obviously thrown from its socket, and the flattening of the shoulder was unusually great. The deltoid was tight, and the elbow was drawn from the side. The arm was lengthened by one inch. The head of the bone rotated with the shaft; the coracoid process was unbroken, but there was distinct crepitus on moving the arm.

The man was very positive in his assurance that the injury was caused by a direct fall on the shoulder. I made an attempt to reduce the dislocation, under chloroform, by the aid of pulleys. I brought the bone towards its natural relation to the acromion process, by extension downwards, with a padded fulcrum in the axilla. The natural rotundity of the shoulder was restored during the extension, but on its remission it disappeared. On the second day following I repeated the attempt more critically, persevering in the extension for nearly half an hour, but with no better success. While under extension the head of the bone became prominent, but returned to its abnormal position on remitting the extending force.

On reviewing all the circumstances of the case, there can be little doubt that the glenoid cavity is separated from the rest of the scapula either in a large proportion or absolutely. A healthy man, possessing the entire power of movement of his arm, sustains a fall directly on his shoulder; the head of the bone is struck inwards with force against the socket, and extreme flattening of the shoulder and crepitus are the immediate consequences; the arm is lengthened, and the elbow projects from the side; the unbroken condition of the head is rendered obvious by its free rotation under pressure; the coracoid process is firm in its position; while two attempts at reduction fail to restore the head of the bone to the glenoid cavity. In what can this injury consist, if not in extensive fracture of the glenoid cavity—the socket of the joint? I believe the fracture to be a large one, because, having seen many cases of fracture of the lower portion of the glenoid cavity, I have never observed one example in which the symptoms were so strongly marked as in this case; while the slipping back of the head of the bone, after being brought on two occasions to its normal position, appears to infer the absence, and therefore the inability of the socket to retain it. The only treatment the case appeared to me to be susceptible of, was that of placing a large pad in the axilla, and leaving the rest to nature; and this plan was adopted.

In cases of a greater or less portion of the glenoid cavity, an easy position of the arm is all that is attainable, and indeed all

that is required. The fragments are usually united in three weeks, during which the arm is placed in a sling.

I had recently another case of fractured scapula under my care in St. Bartholomew's Hospital. This fracture was the result of a fall with violence, but the precise direction of the blow I could not ascertain. The fracture extended vertically through the bone, either through or behind the notch. It was unattended by dislocation, or even by displacement. Its existence was ascertained by the presence of crepitus, which was well marked, and might be detected either by the pressure of the fingers, or by desiring the patient to put into action the trapezius muscle of that side. A large pad was placed under the scapula, which was firmly bandaged to the trunk. His recovery was both quick and complete. I have described elsewhere a case of fracture of the inferior angle of the scapula. The injury occurred to a lady, who was thrown from her horse. In this case I could only obtain crepitus by bringing into action the teres major muscle, which takes its origin from the surface of the scapula, through which the fracture had occurred.

—*Lancet*, *April* 14, 1860, p. 368.

ON THE PREVENTION OF PITTING IN CONFLUENT SMALL-POX.

By William Stokes, M.D., Regius Professor of Physic in the University of Dublin.

[The various modes previously employed, with a view of preventing pitting in cases of small-pox, may be thus enumerated:]

1. The puncture of the pustules when matured.
2. The application of nitrate of silver.
3. The application of oil, or of the *linimentum calcis.*
4. The covering the face with a solution of gutta percha, with collodion, or with glycerine.

[The first of these modes is best adapted to a benign form of the disease, in which the pustules, though numerous, remain discrete for a longer period than in the severer cases. As to the second mode, Dr. Stokes has had no personal experience; it will be most suitable, like the last, in milder forms of the disease.

The third form Dr. S. has tried, but does not consider either application of much value, though the linimentum calcis is preferable.]

During the past five years I have used gutta percha and collodion in a considerable number of cases. These, however, were not by any means examples of the worst form of the disease. In most of them the crust came off in large flakes or patches, composed obviously of the dried exudations and the covering material, and leaving the skin uninjured. To render this treatment effective, at least so far as the exclusion of the air is concerned, it is necessary to renew the application at intervals of from twelve to twenty-four hours; for the covering seems to be repeatedly broken up by the advance of the eruption and the swelling of the parts. Some patients are greatly distressed by the feeling of constriction caused by the coating of gutta percha or collodion, and in general the treatment in question appears unadvisable where there is much vascularity, heat or swelling.

Looking at the frequency of pitting on the face, as compared with that of other parts of the surface, it is not easy to account for it, unless by referring to the fact that, while the rest of the surface is kept covered, and so not only comparatively excluded from the action of the air, but in a state of humidity, the integuments of the face remain in a dry and heated state—first, from the action of the external air, and next, from the increased vascular action. Hard and hot scabs are formed, and the ulcerative process makes its way downwards to a greater or less degree. Some have held that the liability of the face to markings was to be explained by anatomical considerations. However this may be, it will be found that in cases in which from an early period certain portions of the face have been kept protected from the action of the air, and in a permanently moist state, pitting does not occur. This may be seen in cases of sthenic confluent small-pox, where, with the view of preventing the adhesion of the eyelids, poultices have been used over the eyes. In such cases it will be often found that every part of the face is marked, except those over which the little poultices had extended.

The application of poultices over the face appears to me to be

the surest mode of preventing the consequent disfigurement. We should commence their use at the earliest period, and continue it to an advanced stage of the affection. In most cases they may be applied even over the nose, so as to cover the nostrils, for these passages are generally so obstructed as to be for the time useless to the patient. If the nostrils can be kept pervious by injections, the poultices need not be applied over their orifices.

If the chances of marking are in proportion to the activity of the cutaneous irritation, we may hold that this method should fulfil three important indications of treatment—

1. The exclusion of air;
2. The moderation of the local irritation; and,
3. The keeping of the parts in a permanently moist state, so as to prevent the drying and hardening of the scabs.

The value of this treatment, however, will, I feel convinced, be best seen in the inflammatory or sthenic form of the disease. The best poultice will be that formed of linseed meal, which should be spread on a soft material, such as French wadding, and covered with the gutta-percha paper or oiled silk. I have never had occasion to repent the adoption of this practice.

[Dr. Stokes sums up his paper with the following conclusions:]

1. That the chances of marking are much greater in the sthenic or inflammatory, than in the asthenic or typhoid confluent smallpox.

2. That considering the change in the character of disease, both essential and local, observed during late years, we may explain the greater frequency of marking in former times.

3. That in the typhoid forms of the disease the treatment of the surface by an artificial covering, such as gutta percha, or by glycerine, will often prove satisfactory.

4. That in the more active or non-typhoid forms, the use of constant poulticing, and of every other method that will lessen local inflammation, seems to be the best method of preventing the disfigurement of the face. —*Dub. Quarterly Journal*, p. 111.

SUCCESSFUL TREATMENT OF GONORRHŒA AND GLEET WITHOUT COPAIBA.

By Weeden Cooke, Esq. Being an abstract of a paper read before the Harveian Society.

The author commences by stating it as the result of a very large experience, that copaiba in the treatment of gonorrhœa is not only unnecessary, but even, in a great many instances, injurious, and in all offensive to the last degree. As introductory to the subject of treatment, Mr. Cooke discussed the subject of the cause of chordee, the reason for the scalding of the urine, the distinction between true and spurious gonorrhœa, and the time when infection may or may not be apprehended. The conclusions he arrived at were—

1st. The chordee, in ninety-nine cases out of a hundred, was due to spasm, and not to effusion of lymph; and that cubebs, acting as an antispasmodic, formed the most efficacious remedy for this symptom.

2dly. That the scalding was the result of the acid urine passing over the highly inflamed mucous surface of the urethra; and that this was to be remedied by the administration of the alkaline carbonates, for the purpose of neutralizing the acidity of the urine, thus removing the principal cause of the continuance of the inflammation.

3dly. That in all disputed cases, the true gonorrhœa may be known from the spurious by the presence of redness, heat, pain and swelling, together with a purulent discharge, more or less green or offensive; whilst a discharge produced by connexion with a person who has leucorrhœa, or has recently been confined, will not be accompanied with these inflammatory signs and the discharge will be milky in consistency and color, differing much from the thick purulent discharge of the veritable gonorrhœa.

4thly. The time when infection may or may not be apprehended was discussed at its incubative stage, and at its close. A case was given, showing that the disease may be caught from one person, and not communicated to another two nights after, because the purulent discharge had not commenced at the time of the second intercourse. Respecting infection at the close of the

disease, the author had been enabled, from experience, to establish as a law for his own guidance, that gleet—*i. e.*, a mucous discharge from the urethra, consequent upon gonorrhœa—does not set up gonorrhœa in another person; but that, whilst any pus is to be found in the discharge, there is probability of infection.

Passing on to the subject of treatment, Mr. Cooke said that, upon inquiry at the London Custom House, he found that 118,396 pounds of copaiba were admitted into the Port of London only, during the first ten months of the year 1859. If this be administered at the rate of half a drachm three times a day, and supposing each person takes it for three weeks, we have here copaiba enough to treat 473,584, or close upon half a million of persons, and that in ten months only of the year. Considering how often it fails to cure the disease; how frequently it is rejected by, or at any rate, disorders the stomach; how telltale and disagreeable is its odor from the mouth and skin; how occasionally it produces a papular eruption all over the body; and that, in many instances, swelled testicle and stricture may be traced to its irritating influence, while gonorrhœa, rheumatism, and ophthalmia have been attributed to its administration; "considering all these objections," the author remarked, "is it not extraordinary that this disgusting medicine continues so long to hold its ground?" The abortive treatment by strong injections of nitrate of silver had proved a failure, because in some instances inflammation of the bladder had resulted. The treatment by diluents was slow in its action, and not readily employed by persons engaged in active business. That by diuretics was scarcely more successful; whilst the administration of saline aperients was generally attended with an aggravation of the ardor urinæ as well as chordee. The treatment which had been most successful in the author's experience was the chemical treatment by the alkaline carbonates, given with a view of neutralizing the acid in the urine. Thus one great source of irritation was removed from the inflamed urethra, and the subsidence of the inflammation, which nature would effect, was allowed to take place. As auxiliaries, especially when there is œdema of the

prepuce, lead lotions, and elevation of the penis against the abdomen, were commended. The inflammation having subsided and a muco-purulent discharge being left, the author had found, after giving trial to all the injections which have been at any time in vogue, that the chloride of zinc, introduced into this branch of practice by Mr. Lloyd, of St. Bartholomew's, was the most efficacious of any in curing the disease, and that with less discomfort, and in a much shorter time than by any other means. Since employing this treatment, he had had little, if any, orchitis amongst his patients. The strength of the injection he most commonly employed was two grains to the ounce, but in some instances one grain to the ounce is sufficient. Whilst advocating this treatment in persons of healthy constitution, it was necessary to completely change it to others. In the strumous, in the dyspeptic, in those of dissipated habits, and where the diseased person is an old offender, the alkaline carbonates are not called for, because either the urine is not acid or the inflammation does not run high. In such cases the tincture of iron, or sulphuric acid and bark, or gentian, or columba, may be advantageously employed from the commencement; and the chloride-of-zinc injection in these cases is also of the utmost value in rapidly overcoming the disease.

Respecting diet, the author considered that after the subsidence of the inflammatory symptoms scarcely any restriction need be enforced, and that beer or wine in moderate quantities may be advantageously used by those who are accustomed to these beverages. He had found long-established cases of gleet yield readily to the chloride-of-zinc injection, accompanied with tonic treatment and generous living. —*Lancet, Jan.* 28, 1860, p. 92.

Cancer of the Liver.—(By Dr. Harding.)—In many or most cases of cancer of the liver in females, a small subcutaneous tumor will be found in one of the breasts. It is situated just beneath the skin, and is not usually larger than a pea. This little tumor is pathognomonic of cancer of the liver, and may, if discovered in doubtful cases, materially assist diagnosis.

ON HEMORRHAGE FROM THE BOWEL IN CHILDREN AS A SIGN OF POLYPUS IN THE RECTUM.

By Thomas Bryant, Esq., F.R.C.S., Assistant-Surgeon to Guy's Hospital.

[Polypus of the rectum is usually considered a rare affection; but within the last two years at least twenty examples have fallen under the author's notice. The connexion between hemorrhage from the bowel in children, and polypus of the rectum, is remarkably constant. Mr. Bryant says:]

In the majority of the cases which I have observed, the disease has existed for many months; they have all occurred in children under ten years, and in most the disease has been regarded and treated for piles. In some cases the discharge of blood from the bowel is constant, and its buttocks smeared with a bloody mucus. In these instances the polypus will generally be found to be within, if not protruding from, the sphincter. In other examples, occasional discharges of blood will be observed, although not to any very great extent, and this discharge will generally accompany and follow the act of defecation. In others, again, the hemorrhage will take place independently of any such process. There will generally be some straining after stool, but I have never observed any prolapse of the rectum; and although this disease is troublesome to the child, and of course debilitating, by the repeated, if not constant hemorrhage, when once recognized it is easily treated, and rapidly cured.

The recollection that such a disease is not so uncommon, and that it is always associated with hemorrhage from the bowel, should at once lead the surgeon to examine the part with his finger, when the growth will probably be easily detected. It is generally situated about one or two inches up the bowel, and will be found to vary from the size of a pea to that of a large nut. In some cases more than one will be present; they are always very moveable, and easily slip away from the finger upon anything like pressure; and at times some little difficulty is experienced in fixing them for removal. Such a practice is the only correct statement, being invariably followed by a successful result. It may be done by means of forceps or ligature; and

in many cases I have broken the polypus off its attachment by hooking my finger round its pedicle. No bad result has ever followed. The pedicle is always very slender, although it may be an inch or more in length.

The structure of the polypus is very simple, microscopically presenting the ordinary characters of the fibro-cellular growths. After removal no subsequent treatment is required, and recovery may confidently be expected; the rectum, however, should be carefully examined, so that a second polypus be not overlooked.

I have thus briefly brought this small but not unimportant subject before the notice of the profession, feeling confident that the existence of polypus of the rectum is not so uncommon as is generally believed, and that such an affection is usually mistaken and treated for piles. In children, the presence of bleeding from the rectum should at once lead the practitioner to suspect the existence of polypus; and, when detected, its removal is the only correct treatment. I have never had an opportunity of seeing a child suffering from piles, and believe that cases so described are generally mistaken, and that, in reality, they are cases of the disease now under consideration.

—*Lancet, Nov.* 26, 1859, p. 530.

Corns on the Sole of the Foot.—(By Mr. H. Coote.)—Occasionally a small exquisitely painful corn forms on the forepart of the sole of the foot, depending upon undue pressure from slight contraction of some of the important tendons, that most frequently affected being the tendo-Achillis. This contraction of the tendon may be so slight as to need careful examination for its detection, but so long as it exists the cause of the corn remains, and it will be found that any other measure less than the division of the tendon will be only palliative. The tendon must be divided subcutaneously, and the foot restored to its normal bearings.

Eczema of the Hands.—(By Mr. H. Coote.)—M. Guillot recommends the following ointment: lard 20 parts; subcarbonate of soda, oil of cade, tar, of each from 2 to 4 parts.

Editorials and Miscellaneous.

THE NATIONAL MEDICAL ASSOCIATION.

This body met in New Haven on the 5th of June last, and was well attended by physicians from all sections of the country. As usual, most of the time was expended in the discussion of matters which had no special relation to Medicine,—though they seemed real "Godsends" to some of the members, inasmuch as they afforded them an opportunity for the indulgence of that loquacity which seems to be the special failing of many of our medical men.

The subject of Medical Education received a thorough ventilation, and the old contest between the *ins* and the *outs* was renewed with redoubled vigor. We are as warm an advocate for "reform" as any man, but we are not prepared to pander to the insane prejudice which many persons have manifested in this regard,—especially when we believe it has originated solely in selfish and sordid motives. The Report of the Committee on Education—which was read, and prepared by Dr. Reese, of New York—was characterized by that ability and independence which distinguish all that emanates from his pen. It contains many striking facts and valuable suggestions, and was, in every respect, worthy of the subject and of its distinguished author. We voted for its publication with pleasure, and will lay it before our readers when it makes its appearance in print; but we must say that some of the resolutions which received the sanction of the Convention met with our most decided disapprobation, and that, though we stood solitary and alone, our vote was recorded against them. We were not willing to place the Colleges of the country, whatever may be their defects, at the mercy of the selfish and envious cliques which not unfrequently surround them. We could not consent to have the University of Virginia, one of the best schools in this or any other country, deprived of the right of representation in the National Medical Association. We could not sanction a movement which, from its very stringency and harshness, must prove entirely inoperative—"keeping the word

of promise to the ear, and breaking it to the hope," of the Profession; and we felt bound, by every obligation of honor and of duty, to oppose the resolutions at the time,—even though they were the special pets of the Association,—and to expose their fallacies now, however it may prove to their special friends and champions.

Bad legislation is worse than no legislation at all, and we feel assured that the attempts at improvement in Medical Education, recently made by this august body, so far from accomplishing the ends at which they are aimed, will only serve to retard and embarrass the reforms which all acknowledge to be most necessary and desirable.

The best way to insure the entrance of proper men into the ranks of our Profession consists in subjecting those who desire that honor to a thorough, satisfactory and impartial examination; and we feel confident that North Carolina has done more towards the accomplishment of this result, by the establishment of her Board of Independent Medical Examiners, than the Medical Association will accomplish by a century of such legislation as has distinguished it of late. Let other States imitate our example, and the good work will be finished, long before the wranglers who delight in spreading themselves at each annual meeting of the National Convention have concluded their senseless and unprofitable controversies.

SOUTHERN MEDICAL STUDENTS.

We do not desire to introduce political matters into our pages, but we can not refrain from warning Southern men of the reception which they will meet with in the Northern cities, at this particular juncture of our national affairs. A presidential canvass is now being carried on, in which the two great sections of our country are unfortunately arrayed against each other in such a manner as must engender feelings of hostility and bitterness that can not be suppressed. It is true that there are many men at the North who, so far from being unfriendly to us, are really engaged in the noble work of fighting in our behalf against the

base fanatics who would desolate our homes "with fire and the sword;" but this very fact only serves to render the conflict throughout that section of the Union the more fierce and deadly. It only inflames our enemies with a greater hostility towards us. It only makes them the more embittered and malignant in their feelings for their brethren (?) of the South. It renders them the more pointed and personal on the expression of the traitorous sentiments in which they so disgracefully glory in these evil times; and the southerner, whose lot may be cast among the northern people at this particular juncture, though he may meet with warm-hearted and loyal friends, must of necessity be subjected to insults such as his chivalrous spirit can never brook. His ears will be constantly assailed by taunts which must make his blood boil within his veins. He can turn his eyes in no direction but some offensive banner or traitorous inscription will awaken his disgust or indignation; whilst the very fact that he is southern born will make him the object of countless jests, and gibes and indignities, from the despicable wretches around him, such as must involve him in quarrels and conflicts incessantly, if there be one spark of the true fire of manhood in him.

The medical student will be particularly subjected to the indignities and discomforts just described. It is fashionable in the northern cities to ignore his claims to respectability at all times, and to treat him more as an outcast than as a gentleman whenever an occasion presents itself. This is the penalty which he must pay, even at the most tranquil period, for the privilege of attending a northern school. This is the *quid pro quo* demanded under the most favorable circumstances, by those who grow rich upon the annual expenditures of medical classes, in return for the traditional advantages furnished by the institutions of that particular locality. And it does require an extraordinary effort of the mind to comprehend the proposition, that when two distinct causes of discomfort and indignity thus conspire together, the individual who is made the victim of such a combination must, of necessity, suffer to an unusual extent. The man, then, who is despised because he is a student, and hated on account of his southern blood, must have a hard time indeed, whilst attempt-

ing to obtain a knowledge of his profession in a northern school, during the progress of the present exciting political canvass.

Again, the southern student who is thus circumstanced must not only be uncomfortably situated, but it will be impossible for him to pursue his studies to advantage. No man can study to any advantage whose mind is distracted by excited passions, or diverted from the legitimate object of its pursuit by other and more absorbing matters. Tranquillity and concentration are so essential to the student, that there can be no progress without them. Of what advantage, then, can eloquent lectures or successful experiments, or admirable clinics, or extensive hospitals, prove to men whose angry passions are at the boiling point, and whose minds are entirely absorbed in the contemplation of far more exciting subjects? It may be said, that young men have nothing to do with such things; and it is true that, under ordinary circumstances, the southerners at northern schools would not give a thought to politics. But these are extraordinary times, and it is impossible for men to avoid attending to those matters which are *forced* upon them through the medium of their passions or their prejudices; and whether it is proper or improper, there is no disguising or escaping the fact, that politics have, of late, occupied the attention of all classes of our people, almost to the exclusion of everything else.

All know to what an extent the students of Philadelphia and New York were interested in political matters during the last session. Indeed, it is a notorious fact, that hundreds of them returned to the South, whilst the whole body of them was so much excited in regard to the subject that there was little or no studying done for weeks. Now, without considering the right or the wrong of the secession movement which then took place, we desire simply to say, that if the North was found so disagreeable to southern men then—if it was so difficult for students to attend to their legitimate duties during a period of comparative tranquillity,—what may those expect who patronize northern schools at a time when the flags of abolitionism are floating from every hill-top, when the defiant shouts of traitors are resounding throughout the entire North, and when fanaticism

stands with torch already lighted, waiting in anxious solicitude to apply it to our dwellings?

We are no disunionist. We believe that there are conservative men enough in the country to save it. We are convinced that the great struggle which is now agitating the country is to terminate in the complete overthrow and final discomfiture of the miserable traitors who have plotted the destruction. But we must say, that whilst the struggle is being decided, whilst the contest is still going on, southern men had better keep away from the battle-field, unless they visit it to take a part in the fight.

The time has, in fact, arrived when southern men should encourage the South,—when they should aid in fostering her interests,—when they should assist in sustaining her institutions,—when they should delight in contributing to her prosperity and her glory. Her medical institutions already compare most favorably with those of other sections, and if the students who annually flock to the North would but pause to examine their claims to patronage, they would soon learn that interest unites with patriotism in supplying them with a motive to encourage the medical schools of the South.

The delay in this number of the Journal has been totally unavoidable. The protracted illness of the publisher caused its delay, and it is hoped this will be a sufficient excuse. We have now procured a new printer, and the next number will appear early in November.

Fistula-in-Ano.—(By M. Bonnafont.)—Cases of fistula-in-ano are now treated in the Parisian hospitals by injections of strong tincture of iodine, according to the plan followed so successfully by our own surgeons. It is a good plan, before injecting, to introduce a tent into the bowel to absorb the superfluous liquid and prevent irritation of the mucous membrane.

NORTH CAROLINA

MEDICAL JOURNAL.

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Original Communications and Translations.

CASE OF INGUINAL HERNIA, IN WHICH THE CŒCUM WAS FOUND IN THE SAC.

BY THOMAS S. DUFFY, M.D., OF RUTHERFORDTON, N. C.

I WAS called, on the 5th of August, to visit a muscular mulatto man, aged 23, belonging to Mr. Long, of Whiteside Settlement. He had been ruptured for the first time about three months previously. On the 3d, being election day, he walked eight miles to an election precinct, and back. Next day (a kind of jubilee with the negroes in this part of the State) he walked to town, fourteen miles, where, although troubled with colic, he was imprudent enough to drink cider, eat melons, and engage in violent exercises. In making a great effort, while wrestling, the bowel escaped, and all his efforts to return it were useless. In this circumstance, instead of applying for assistance in town, he endeavored to go home in an ox-cart, and succeeded in getting twelve miles over a very rough road. During the journey he suffered extreme pain. At the house of Mr. Hamilton, where he stopped, laborious efforts were made to relieve him, but without avail. During the night stercoraceous vomiting came on, and then medical assistance was called in. When I saw him, twenty-two hours had elapsed since the accident. The abdomen was swollen, and so sensitive, he could not well bear it to be examined. The pulse was weak, but frequent—the tongue white and coated. The tumor was large, tense and unyielding. I tried the taxis as

far as prudence would justify, but without any result, and the operation became a matter of necessity.

In dividing the hernial coverings, the fibres of the cremaster muscle were found to be distinct and fleshy. Much care had to be observed in cutting through the peritoneum. Owing to its firmness and the pressure from within, it was found impossible to pinch up a portion with the forceps. An opening was made, without injury, sufficient to admit a director, and the incision completed to the required extent. About three ounces of dark colored fluid, with a highly offensive fœcal odor, escaped. The contents of the sac occasioned a good deal of surprise. They consisted of the appendix vermiformis, cœcum, and about seven inches of the ascending colon, all in a high state of discoloration and congestion. The appendix was the most prominent object, lying directly in front. The inflation was so great, it was hard to introduce the knife safely; but with the judicious assistance of my friend, Dr. Leventhorpe, this was effected, and the stricture divided. But the difficulties were not yet over. The recti muscles were as rigid as in cases of tetanus; and this condition, together with the swollen state of the parts within, rendered it impossible to replace the extruded mass. I now enlarged the external incision upwards, and having ascertained the position of the epigastric vessels, the knife was once more introduced, and the tendon of the obliquus externus, with the fleshy margin of the internal oblique and transversalis muscles, divided to the extent of an inch. An injection, containing salts and spirits of turpentine, was then thrown up, and I was enabled soon afterwards to return the bowels. The wound was adjusted, and the patient put to bed, where he expressed himself as feeling quite comfortable. This was at 2 o'clock P. M. When he had rested half an hour, fifteen grains of calomel were given him, and a few doses of morphine throughout the afternoon.

Six o'clock.—He is resting. Expression of countenance good. Pulse fuller, and not so quick. Swelling and rigidity of abdomen continue the same. The bowels have not been moved. A purgative injection ordered, and the morphine mixture to be given at intervals of three or four hours.

August 6th, A. M.—He has passed the night pretty easily—no particular change. Poured warm water over the dressings previous to removing them. This was succeeded with an urgent desire to go to stool. He voided, with great force, an immense quantity of the most offensive semi-fluid matter of the color of gunpowder. The old dressings were removed, and replaced with fresh ones—the cloths to be kept moist with warm water. At this stage of the case I had to return home. I learned afterwards that the patient commenced sinking about 2 o'clock P. M., and died at 8.

A CASE OF STRANGULATED UMBILICAL HERNIA WITHOUT A SAC.

BY D. T. TAYLOE, M.D., WASHINGTON, N. C.

ELIZA, a mulatto, when quite a young girl, suffered from chronic ulceration of the umbilicus, produced by the pressure of an oak split worn in her corsets. This ulceration was extensive, and resulted in an issue which continued open for a period of seven or eight years. During the last three years of her suffering from this affection, *at every monthly period the menstrual fluid was discharged through the umbilical opening.*

By a long course of treatment the issue was finally closed. The healing process was perfect, but the umbilicus remained enlarged in circumference, somewhat protuberant, and corrugated. Her health was good for five years or more, and there was no complaint of pain or weakness at the seat of her former disease. This is a brief history of the patient up to the time I was called to her, on the 24th of November, 1859.

I found her in a cold, clammy sweat, with a thread-like, hurried pulse, intense pain over the abdomen, continual vomiting, distressed countenance—in short, *all* the symptoms usual in strangulated hernia. On inquiry, I learned that she had lifted a child suddenly, "had felt something give way," and had suffered intense pain ever since. Some four hours had elapsed since the occurrence of the accident. On examination I found a knot of intestines, as large as my fist, hanging down nearly to

the pubis. The hernial protrusion was without any peritoneal covering.

The intestine was in a highly congested state, very red, and from its entire surface was oozing a bloody, watery fluid. I soon found it impossible to reduce the hernia by the taxis. The operation was a simple one. An incision above and below the protrusion, through the enlarged umbilicus, and the intestines were returned without any great difficulty. A couple of stitches, a few adhesive straps, a compress and supporting bandage, and my patient was comfortable. Eight months have now elapsed, and the patient is in the enjoyment of her usual health.

On Quinio, or Rough Quinine.—(By M. Batka.)—A substance is known in the Brazils under the name of quinio, which is extracted from the fresh bark of the cinchona by lime, and then from the lime by alcohol. It is very rich in quinine, and it is only necessary to boil it with dilute sulphuric acid to obtain an abundant crystallization of pure sulphate of quinine. Quinio is a yellow body of a resinous appearance and of a bitter taste. It is insoluble in cold, and but slightly soluble in boiling water. It is very soluble in alcohol and ether, separating partially from the latter by exposure to the sun. Water precipitates the alcoholic solution. It is almost entirely soluble in weak sulphuric acid, from which soda precipitates it of a dirty white color, the precipitate assuming the appearance of a resin. A beautiful white sulphate, however, may be prepared from it. Quinio is free from cellulose; when heated, it gives off an odor something like cinnamon, and burnt, leaves a light residue of carbonate of lime. It resembles a good deal the quinoidine of Liebeg, but is much purer than quinoidine of commerce.

—*Dublin Hosp. Gaz.*

Spermatorrhœa.—Dr. Laroche (Bull. Gén. de Thér.) records a case of spermatorrhœa treated with success by digitalis, as recommended by Corvisart. The dose is one or two grains of the powder of the plant, gradually elevated to eight grains.

SELECTIONS.

Practice.

A CLINICAL LECTURE ON THE DISTINCTIONS OF TYPHUS AND ENTERIC (TYPHOID) FEVER.

By W. T. Gairdner, M.D., Physician to the Royal Infirmary of Edinburgh. Delivered June 5th, 1860.

Gentlemen,—I am going to say a few words to you upon a subject of great importance, which we have not, till now, had a proper opportunity for discussing for months past—the distinction of the Enteric, or, as it is often called, "Typhoid" Fever, from the form so long and so well known here under the name of Typhus. And as I observe that the doctrines which I have for some years regarded as established, and taught as being beyond reasonable doubt, have quite recently been again brought into question in the Medical and Chirurgical Society of London, I will ask you to observe very carefully for yourselves whatever facts our own experience affords as bearing on the subject.

Since the session began (in May) you have seen me pick out four cases from the miscellaneous list of febrile diseases sent up to the female fever ward; and these four cases you have heard me pronounce to be cases of enteric fever, on the faith of certain diagnostic peculiarities which have been described by various observers, and especially by Louis; but of which I must confess never to have had quite a clear idea until the publication of the researches of Dr. Jenner, of London, in 1848. Founding partly on the results obtained by Dr. Jenner, and partly on my own limited experience since 1848, I have never hesitated for some years past to point out the cases of this disease as differing *essentially* from cases of typhus fever—*i. e.*, differing as small-pox differs from measles, or both of these from scarlet fever, or from typhus. In other words, I have been led to regard this enteric fever as a *specific* type of disease, not, perhaps, always distinguishable in practice from all others, any more than scarlet fever, and even small-pox, are always distinguishable; but when well

characterized, always observed to follow a law of development and diffusion which marks it out as an independent disease, not confounded in *nature* with any other fever. Now I wish you to observe particularly, and therefore not to be confounded *in name*, in connexion with these four cases, on what grounds I have placed this diagnosis; what doubts we have admitted in making it, and how those doubts have been solved. I may mention that all the four cases are now convalescent; so that I hope we shall not have in any of them that kind of confirmatory evidence which consists in finding, after death, ulceration of the small intestines, so characteristic of this kind of fever.

Observe, now, that judging from our four cases, the characteristic phenomena of this fever are not to be found either in its duration, or in its severity, or in its so-called "typhoid" characters. The presence or absence of delirium, or of stupor, or of dry tongue, affords no ground of distinction. One of our cases had severe and protracted delirium, approaching coma; another had no delirium, but was very deaf and apathetic, with a foul and dry tongue for many days; the other two had hardly any "typhoid" symptoms at all. The heat of skin was equally variable; in one it was scarcely ever much elevated; in all the others it varied greatly at different periods, as also did the pulse. In not one of the cases was there a rapid and decided crisis; but one began to amend about the eleventh day, another about the twelfth day, while the two remaining cases were excessively protracted, and can hardly be said even now, after nearly seven weeks, to be out of danger; the fever having, in one of the two, degenerated into a kind of hectic type, exceedingly like that of subacute phthisis—a consequence, let me remark, which is very much more to be dreaded in enteric fever than in any other form of specific fever with which I am acquainted.

Nothing, then, can well be more variable or less characteristic than the general symptoms of this fever. I have seen it resolve itself in ten days, with the symptoms of febricula only, or of a mild remittent fever; I have seen it, on the other hand, last nearly as many weeks, and pass imperceptibly into organic disease. It mimics, in turn, not only all other fevers, but many

other general or local diseases—phthisis, pneumonia, meningitis, perhaps more frequently than most others.

Next, as regards complications, we find that in two of our four cases there was intense deafness; in one, if not in both, with disorganization of the ears. In the same two there was pulmonary complication of considerable intensity, and doubtful issue; in one of them resembling broncho-pneumonia, in the other tubercular disease. The other two cases were almost perfectly free from pulmonary or other severe complication, and but for a little diarrhœa at the commencement, might have been said to be free from complication altogether.

But *there was diarrhœa, more or less, in all the four cases.* This, I need hardly say, is the particular symptom from which the name "enteric," as applied to this fever, is derived. Diarrhœa, without known cause, with light-colored, ochrey, or blood-tinged stools, and with a tympanitic state of the abdomen, gurgling on movement, or tenderness on pressure in the right iliac fossa, are symptoms as nearly characteristic of this fever as any that can be named. And yet I would not have you to trust to these symptoms too much, for some of them are absent in the majority of cases: and even diarrhœa may be very transitory, as it was in two, at least, of our cases (not lasting beyond a few days, or even hours); or it may appear to have been determined by a purgative. Further, there may be no diarrhœa; and even in fatal cases, with great ulceration of the intestines, diarrhœa may not appear till a very late period. Besides, in typhus fever, and, indeed, in all fevers whatever, diarrhœa, as an incidental symptom, is by no means uncommon.

Now observe, further, that *in all our four cases there was a peculiar eruption on the skin.* This, when present, and in sufficient amount to be identified, is the great criterion of enteric fever, as the eruption of typhus is also of that disease. I will not dwell on the characters of these eruptions here in the class-room. I will ask you to study them carefully in the wards whenever you have opportunities, and particularly to contrast them with one another. Of the maculated eruption of typhus, you have two excellent examples under observation at present.

I will only recall to your attention now the fact, fully demonstrated to you before on many occasions, that in most of our cases of enteric fever the eruption has been very scanty, and that we have not unfrequently hesitated for a day or two to make a diagnosis. Indeed, the first crop of the eruption is rarely quite decisive; but as soon as successive crops, even of two or three spots each, appear, all doubt is removed. But in order to be sure, it is usually requisite in this disease, when the eruption is scanty, to mark each individual spot with ink in such a way as to distinguish accurately the period of its appearance; which precaution against error, accordingly, you saw me take in all these four cases. This, in typhus fever, it is usually unnecessary to do; and from the number of the spots, it would be impossible even if it were necessary.

As regards the characters of these eruptions, I may refer you for details to the excellent descriptions and illustrations of Dr. Jenner in the thirty-third volume of the "Medico-Chirurgical Transactions," with which I entirely concur.

As regards the characteristic phenomena of typhus, and particularly the relation of the eruption to these phenomena as we have lately observed them in Edinburgh, I have no time to enlarge at present, and I will therefore take the liberty of referring you to a short paper of mine in the number for July, 1859, of the "Edinburgh Medical Journal," on the changes that have occurred in our fevers during the last ten or twelve years. You will there find that typhus, like enteric fever (though not certainly to the same degree), is a disease of exceedingly variable severity and duration; that its general febrile characters, though following on the whole a certain type, are by no means to be depended on in individual cases; that its complications are few, and follow no regular rule; and that, therefore, still more decidedly than in enteric fever, we have usually to fall back on the eruption for its diagnosis.

You will observe, then, that in the diagnosis of enteric fever, as compared with our old and familiar (but, happily, not *of late* very familiar) typhus, I rely not so much on any of the characters of the fever itself, though these are often very peculiar and

distinctive when taken all together; but chiefly, and in some cases exclusively, on the eruption. And the eruption I believe to be characteristic thus far, that when sufficiently abundant and well marked, it shows forth quite decidedly which of the two diseases is present. When scanty in amount, and present only for a short time, it sometimes leaves us in doubt, not only whether the fever present is typhus or enteric, but whether it is an eruptive or specific fever at all. Thus you saw that, in one of our four cases, I remained in suspense for two or three days, but was afterwards quite decidedly enabled to pronounce that the eruption was that of enteric fever. And as regards typhus, some of you may have observed that, in the two cases now in the ward, I did not for a single instant admit of a doubt. The eruption, being a well marked one, was to my mind a conclusive evidence of the nature of the disease, without a single inquiry as to the symptoms or antecedents of the cases. The same perfectly distinctive character of the eruption, at the first visit, has often occurred to me in the case of enteric fever.

Now, I presume you will ask, and you certainly will do well to ask, what proof can be presented that the two eruptions alluded to (admitting them, as I think you will do, to be distinguishable from each other in the great majority of cases) indicate two diseases, and not mere accidental varieties of one disease? I have already fully confessed to you that, unless the eruption makes the distinction, there is no sufficient and constant distinction in the symptoms ascertainable during life. Of course there is the additional *fact* of the ulceration of the Peyerian patches—by far the most noteworthy single fact, pathologically speaking, in the natural history of enteric fever. But it will not do to found on a fact like this in the matter of diagnosis; and, besides, after you have settled that there are cases of fever with ulcerated intestines, you have still to settle whether these cases are different in nature from other cases, in many respects similar, in which the intestines are not ulcerated.

Have we, then, here before you *two distinct fevers with two distinct eruptions, or only one fever with two varieties of erup-*

tion?—that is the question. Let us look at it, first, in the light of the six cases at present in the ward.

Two of these cases, I have already told you, were recognized, not by a careful investigation of symptoms, but simply by a single glance at the eruption, as cases of typhus. What is the history of these cases? They are two sisters of a family of five—namely, a mother, three daughters, and a son,—who, with a lodger, occupied a house in St. Mary's wynd, in the immediate neighborhood of a large tan-work. Of these six persons,* five have had fever; and we know positively that in all these it has been typhus with eruption. The brother and the lodger you may see for yourselves in the male fever ward; the mother was the very last case of typhus we had under treatment in our own ward, and it is now more than a month since she died (May 1st). We do not know whence this fever originated; there is some ground for suspicion that the mother was visiting a fever case in another ward in the hospital before she was attacked, but as I cannot trace the story distinctly, we had better not insist upon it. Be that as it may, we have here evidently a group of five cases having a common contagion; and all of them prove to be typhus with distinct typhus eruption. Excepting this group, there has not been a single case of typhus in the Infirmary, I believe, since the 25th of April, and then only one, from the New Town. In March, there was only one case—a girl from Dalkeith. In February, there was only one case—the last of a little group which, like the present group, was composed of typhus cases only. In January there were one or two other cases. In the Dispensary books I find no case of typhus recorded since March, except one of those which I have noticed as admitted here. So that for three months, at least (we may say confidently), Edinburgh has been almost clear of typhus fever, with the exception of the cases now enumerated.

As regards the four cases of enteric fever, I find that three of

* Another daughter, the only remaining person of this group of six, has since passed through typhus fever with distinct eruption, under our observation (July 4th).

the four are apparently quite isolated cases. This is in accordance with what we know of this disease, which has much less tendency than typhus to run into groups of cases, and is, I believe, much less contagious; but in the fourth case we have got hold of a link in a chain or group of cases, and this time the locality is not in Edinburgh, but in Penicuick, a village at nine miles distance. I can tell you nothing about these cases from my own experience; but, fortunately, they were seen by Dr. Thin, of Penicuick, who sent in the girl *as a case of enteric fever*, and assures me that her brother had the same fever, with a like eruption, in a severe form, and that other cases of the disease occurred in the same house. And I know that Dr. Thin's observation is most implicitly to be trusted upon this subject, because he was a most valued pupil of mine a few years since, and had ample opportunities of studying fever during his attendance in the Royal Infirmary. Nothing short of having seen these cases with my own eyes, could give me more confidence than this in telling you that they actually were cases of unquestionable enteric fever. Now, besides the cases under observation at present, I find recorded in the book of the fever wards only one case of enteric fever in May, two in April, one in March, one in February, two in January—all, so far as known to me, isolated cases; and, further, I find the Dispensary books quite void of any reference to the disease.

Here, then, is the kernel of my argument. Fever has not been epidemic in Edinburgh for six months at least; on the contrary, it seems probable that there have been hardly a dozen cases of typhus in all, and about as many of enteric fever, in the entire town during the whole of that period. But five cases are found in a single household, and these are all, without exception, cases of eruptive typhus. One case of enteric fever is sent in from Penicuick, and forms part of a little epidemic or local visitation there, which, we are assured by Dr. Thin, consists of enteric fever only, and not at all of typhus, as distinguished by eruption. The two diseases are, therefore, as distinct as they can possibly be. No case of typhus has given rise to anything

but typhus; no case of enteric fever has given rise to, or been associated with, anything but enteric fever.

Now this is only a small contribution to a kind of experience of which Dr. Jenner has availed himself, with great care and exactness, in the much greater field of London. Here, again, therefore, I may refer you to his paper, already mentioned, for further details. But there is this advantage in a small field, that you can be much more sure of exhausting your facts. No one can pretend to have had access to all, or nearly all, the fever cases of London, during however short a period. But in Edinburgh, Dr. Begbie and myself probably have seen, or have had the means of knowing about, very nearly all the fever cases; and, therefore, when I declare to you that, within my experience for ten years past, no instance has occurred of a decided origin of enteric fever in a group of typhus cases, or of typhus fever in a group of enteric cases, I am entitled to say that I have obtained very strong evidence in corroboration of the view, that these two diseases are, in reality, different diseases, and not mere varieties of the same disease.

Last summer I made a very careful survey of the whole fever-field of Edinburgh (if I may call it so) for several months together. It was not an epidemic season; but I gathered about thirty cases of typhus and twelve of enteric fever, and into the whole details of these I inquired with the greatest possible minuteness, visiting every one of the fever localities, except one or two in which I was quite sure the cases were isolated. The result was, that in no case could I light upon a suspicion that typhus had given rise to anything but typhus, or enteric fever to anything but enteric fever. The details of this inquiry you will find in the "Edinburgh Med. Journal" for September, 1859.

We had, however, in the Infirmary a melancholy proof that although typhus cannot give rise to enteric fever, or enteric fever to typhus, it is possible for persons who have passed through enteric fever to take typhus very soon afterwards. Last summer we were unable to avoid, during a certain period, the association of the two diseases in the same ward in rather excessive amount. Now, mark what followed. No case of typhus was seized with

enteric fever; but no less than four of the dozen cases of enteric fever were attacked within a few weeks with typhus, and I am sorry to say that the mother of a very interesting family of five (who all, except the father, passed through enteric fever) died of this secondary attack of typhus, caught, beyond question, from typhus cases in the ward where she was visiting her daughters. Both these daughters afterwards sickened of typhus; but although one of them was excessively debilitated, we succeeded in saving both. Another young girl had the two diseases in succession, but in each case mildly.

I beg you to notice very particularly these facts; for not only are they full of instruction as to the danger of associating typhus cases with enteric fever, or indeed with any other fever in the same ward, but they form the most conclusive of all possible proofs that the two diseases are distinct diseases. Typhus fever is not subject to a relapse; and it is a curious and indisputable law with respect to it, that it almost never attacks again, at least within a period of years, those who have had it before. Yet here we have three members of one family, within a few weeks, seized with two distinct febrile attacks, with two distinct eruptions, the attacks being separated the one from the other by a perfectly distinct convalescence. To make these out to be mere varieties of typhus fever, would require the whole laws of that fever, as ascertained by innumerable observations here and elsewhere, to be set at naught in this particular instance. —*London Lancet.*

MEDICATION, OR NON-MEDICATION? THAT IS THE QUESTION.

BY BENNET DOWLER, M.D.

"In regard to the employment of the experimental method, we stand much upon the same point as that upon which chemical experiment stood in the times of phlogiston, or even of alchemy; and as the phlogiston hypothesis so long stood in the way of every more exact investigation so *our* attempts have been impeded by the old hypotheses of an 'archæus' or a 'pneuma,' and by the more modern ones, of a vital force, a typical or healing force, and, lastly, by the so-called ontological conception of diseases as special entities, no less than by the belief in mystical medicinal forces."—F. OESTERLEN, M.D.—*Med. Logic*, p. 262.

THE recent unexpected and much lamented death of Dr. Todd, of London, just as he had completed his new book on "Clinical Lectures on Acute Diseases," seems to have drawn much attention to this work as the last will and testament of a man whose

previous career of authorship had been alike useful and illustrious. Although there may be but little merit in praising a dead competitor, yet it is but just to say, that the living not less than the dead Dr. Todd seems to have been highly appreciated at home as well as in this Republic. It is, however, difficult to account for the special eulogy and sanction awarded to his last work by practitioners, teachers and reviewers, seeing that it utterly denies the existence of acute inflammations, acute diseases, and sthenia, and interdicts antiphlogistics altogether. Dr. Todd is not, however, a strict non-medicationist; he has a remedy, if not a panacea. It is, O Sons of Temperance!—rejoice, O ghost of John Brown, of Edinburgh!—it is alcohol, brandy, wine. Leave the rest to Nature. The poet but followed the faculty of former days in his anathema—

> "To those whom fever burns, the smell
> Of vigorous wine is death and hell."

Of Dr. Todd's book, which is devoid of novelty, originality and sound logic, it is not intended to make any special mention, further than that it seems to have given an almost epidemic impetus to skepticism. It is easy to see that its chief eulogists are delighted with, and emboldened by, its authority. In this defection, it is asserted many of the younger members of the profession are included. Doubtlessly, the great majority will never join in the revolt, but imitate Milton's

> "Abdiel, faithful found
> Among the faithless—
> Among innumerable false, unmoved,
> Unshaken, unseduced, unterrified,
> His loyalty he kept, his love, his zeal;
> Nor number nor example with him wrought
> To swerve from truth, or change his constant mind."

In making a few unpremeditated and unsystematic remarks upon the existing skepticism in regard to medical treatment, doubtlessly the strict logician may discover some errors and inconclusive arguments; but if the leading postulates be on the whole sustained with more or less probability,—if there be no undue biases in favor of untenable theories, or over-estimates of the valid claims of therapeutics—no malice towards those who

wholly repudiate these claims, the reader will, it is hoped, excuse the attempt to defend the truth, though it may not be in all respects successful.

Dr. Todd's rejection, *in toto*, of the doctrines of sthenia and antiphlogistics, is received with acclaim by the skeptics. These terms may or may not be pathologically correct. Sthenia and asthenia, though convenient and useful words, are of hypothetical import in pathology, if literally applied. The very excess of sthenia is often the parent of asthenia. It is, however, impossible to provide accurate terms for that which is but imperfectly known. Even active and passive hemorrhages differ, not in their nature, but degrees. Active medication has no antithesis which can be conceived, much less practised, as being medication at all.

The old doctrine of phlogiston has given impetus to the hypothesis that there is a fundamental class of medicines which are antiphlogistics and constitute a method of cure, namely, the antiphlogistic method or system—which, as all know, a few years ago reigned with unrelenting despotism. The medicinal agents which were given under this name in diseases called phlegmasial, phlogistic or inflammatory, because of their real or supposed efficacy in removing inflammation by acting antiphlogistically, became a class, and every cure thus achieved was supposed to prove the correctness of this classification; thus doctrine, language and therapy mutually confirmed each other, and a mere hypothesis was regarded as the deduction from rigid science. What has been gained in therapy by labored arguments for and against the antiphlogistical character of quinine, mercury, opium, etc? The using this word either to designate a class of medicines or a method of treatment, may be convenient, rather than correct. If brandy in certain cases of fever or inflammation should, as it may, lessen the disease, it would be entitled to the character of antiphlogistic as well as venesection; and so of other remedies among stimulants, and tonics, and diet.

After writing this last paragraph, the following *jeux d'esprit*, half argument, half irony, by M. Forget, was met with in the "Medical Times and Gazette" of last June, and will be subjoined,

although its import is little favorable to drugs and druggists, and their prescribing patrons, the doctors:

"Dr. Forget on Antiphlogistic Treatment.—Broussais invented inflammation and bleeding, it seems. Hippocrates, Celsus, Galen, never suspected the existence of inflammation. MM. Andral and Gavarret are only the satraps of the reformer; and as for bleeding, this same Galen, and Sydenham, Botal, Guy-Patin, Chirac, Sylva, Hecquet, Bosquillon, and *tuti quanti*, never used the lancet. Hence, then,

"'Si nous saignons sans succès,
C'est la faute de Broussais.'"

Unable to destroy inflammation, we have begun to calumniate it; it has completely changed its nature, we are told. For three thousand years it has been considered as an *exaggeration* of the vital forces, but we have it now demonstrated as a *depression* of the nervous system. For three thousand years bleeding has been used as the most natural means of disgorging the inflamed tissues, and now it is irrefragably proved that bleeding aggravates and produces inflammation. We have not only theoretical proofs of the fact, but we have practical proofs, which no one will refuse—clinical experiment. Thus English, German, and French practitioners have taken it into their heads to lay the bridle on the neck of the phlegmasiæ of pneumonia and erysipelas, for example; some of them have the courage even to treat them with alcohol, by the aid of which their patients do admirably. Ah! you will say, that does not surprise me at all; we have had expectant doctors, like Hippocrates; and busy doctors, like Asclepiades; and debilitating doctors, like Sydenham and Broussais; and stimulating doctors, like Morton and Brown. At all times there have been empirics and methodists, who have, turn by turn, ruled the medical world. Broussais dethroned Brown, and now behold Brown revenging himself on Broussais. Science, like representative government, is a system of balancing. The truth of to-day is the error of the morrow. All which proves that there is no constancy in Nature, as Bichat has said, and proves also that Nature is stronger than physic and physicians; for if she were the slave of systems, the world would

soon be a desert. But statistics? you will say. Statistics are always on the side of the party who invokes their aid; they are kindly handmaids, at the service of the first comer. The value of statistics is the value of the observer. Science, good sense and honesty are of more worth than figures."

It appears from an interesting and finely illustrated memoir, for which the Academy of Sciences lately awarded M. Forget a prize, (a memoir kindly sent to the "New Orleans Medical and Surgical Journal"), that while M. Forget says, "I am a partisan, pathogenically, of the doctrine of naturism," yet the very first case reported in his work he calls an "ulcerous inflammation," and the treatment first adopted is called antiphlogistic. He says: "In November, 1854, a violent inflammation occurred. Antiphlogistic treatment was employed, two applications of leeches were made, and the inflammatory symptoms decreased." (*Dental Anomalies and their Influence*, pp. 7, 8, 28.)

Sthenia and antiphlogistics, I may further remark, are innocent and legitimate babes, compared with numerous phrases and terms in use, which, however, serve admirably to conceal our ignorance of certain pathological and therapeutical topics, as nervous, reflex, dynamic, etc., among which is one now becoming axiomatic, as if it were as clear as that a part is less than the whole—namely, that pathology is nothing but diseased or deranged physiology; symptoms, pathological anatomy, all quantitative, qualitative, all dynamical and material aberrations and changes being purely physiological. But if it were expedient to look very closely into this seemingly lucid explanation, it would soon appear that there is more darkness than light in it. One might as well define falsehood by deranged truth, insanity by deranged sanity, vice by deranged virtue; all of which are antitheses, being neither analogous nor identical. Whatsoever is morbid is a constant recession from, and incompatible with, physiological function. Sickness is not health. Whether we rely upon what we observe in the ordinary maladies of people, or produce, artificially, that is, by experiment, diseased conditions, it will be seen that every morbid symptom or change is just so far a departure from the normal physiological actions which constitute

health. Physiological sickness and physiological venesection, or drugs, as opiates, quinine, blisters, etc., do not, at least, appear to be very happy examples of medical nomenclature. The same remark may be applied to those writers who mention that sensation is sometimes not perceived or felt. Now, any sensation which is not felt, is not. The feeling of a sensation is the whole of it, and essential to its very nature. Hearing, seeing, smelling, tasting, pain, anger, which produce no sensation, have no reality for the individual, neither for the judge nor jury, and could not be received as evidence in any court.

According to the new, or rather according to the old sect now revived, "Nature healeth our diseases:" what may this goddess be? Is she material or immaterial? mind? or matter? or both? The German pantheists believe that there is no God other than the universe, the whole of matter and mind. Pope's idea is, that

> "All are but parts of one stupendous whole,
> Whose body Nature is, and God the soul."

One philosopher maintains that phenomenal Nature is a self-revelation of the Deity, while another announces that Nature conceals God!

Mr. Stallo, in his work on the "Philosophy of Nature," (or one might call it the Philosophy of Germany), gives the following exposition of Nature and her laws: "The teleological system, or the doctrine of *causæ finales*, with its laws, for which, on account of their formal generality, it was impossible to give any empirical [experimental] warrant [are]: the *lex parsimoniæ*, that Nature attains her ends by the shortest possible route, and by the fewest possible means; the *lex continui*, that Nature never proceeds *per saltum*, either in the succession or the coärrangement of her products; the *lex subsumptionis*, that Nature's variety reduces itself to a few principles."

The student of history, from its earliest records to the present day, will have found that there have been all sorts of gods and goddesses relied on or appealed to in the science and art of healing, as demons, astrologers, conjurers, witches, and in our day homœopathists, spiritual table-turning doctors, and many other sects.

The revival of non-medication (if a negation may be revived) and the rediscovery of Nature, and that she alone can heal the sick, without drugs—or, as others affirm, with the aid of alcohol, full diet, etc., are doctrines which have been avowed by not a few men of learning and moral excellence. Have these men received a revelation from Nature or any other goddess? This they neither can nor do avow. But they virtually claim to know the intentions and conduct of Nature better than the great mass of medical professors and practitioners of all nations and of all ages, not to mention the non-professional world, not one in a thousand of whom doubts the efficacy of medication. Now, all of these classes have derived their opinions from the same Nature and Art in disease which the skeptics themselves rely upon to prove the mischievous effects of the medication, however skillful. Now, this overwhelming majority who attest the efficacy of medication have at least an equal opportunity of knowing all the facts necessary to the formation of an enlightened opinion, and of drawing rational deductions from both personal observations and experience, and the experience of all ages, times and places.

There never was a time in the history of legitimate Medicine when either the physicians or the people generally supposed that medicinal agents, judiciously applied, were always injurious or wholly inefficacious in relieving from suffering or in removing disease and preventing untimely death. Now, the speculative opinions of the few cannot overthrow the speculative opinions of the many, in matters where all are equally qualified to judge, having equal opportunities to observe. Under these circumstances, in a matter purely experimental, the opinion of one man cannot outweigh the opinion and experience of a million, especially when fortified by all traditional and historical testimony. That emetics, cathartics, stimulants, opiates, etc., have been valuable remedial agents, when given with ordinary judgment and skill, all history testifies.

We have no evidence but that of the skeptics themselves showing that they have peculiar and indisputable claims to in-

terpret the purposes, plans and powers of Nature, nor that they alone have a knowledge of what she can and will do.

That Nature, or, as some have it, God, is a doctor, has not any foundation in revelation.

The very first book of Sacred History, Genesis, recognizes doctors: "Joseph commanded his servants, the physicians, etc." Throughout the whole series of Sacred writings, no reference is made to a metaphysical abstraction, or myth, as being the only doctor, called Nature—the most ambiguous word known in human vocabularies; while, on the other hand, physicians and their vocation are often mentioned. Pious and rational doctors of modern times, who say "God healeth our diseases," intended to say that the means of cure, or the functions of the physician, were also implied or enjoined in the process, just as much as industrial means are implied in producing corn, clothing, printing, gunpowder, telegraphs, railroads, etc.

The learned Dr. Renouard, of Paris, in his recent work, the "History of Medicine" (translated by Professor Comegys), makes the following quotation from the Ecclesiasticus, a book indubitably very ancient, and held to be canonical by the Catholic Church, and generally bound in the Protestant version of the Bible, between the old and New Testaments. I have added from the same chapter referred to by Dr. R. several additional passages:

"Honor a physician with the honor due to him, for the uses which you may have of him. For of the Most High cometh healing, and he shall receive honor of the king. The skill of the physician shall lift up his head; and in the fight of the great men he shall be in admiration. The Lord hath created medicines out of the earth; and he that is wise will not abhor them." (Chap. xxxviii. 1, 2, 3, 4.)

The following, from the same sacred record, is a true exposition of the manner in which the "Lord healeth": "He hath given man skill, that he might be honored in his marvelous works. With such doth he heal men, and taketh away their pains. Then give place to the physician, for the Lord hath

created him; let him not go from thee, for thou hast need of him. There is a time when in their hands there is good success." (V. 6, 7, 12, 13.)

In the eighth verse, the apothecary and his pharmaceutical preparations are alluded to: "Of such doth the apothecary make a confection," etc. Whether this confection was like any of the numerous formulæ of the American Dispensatory, as the *confectio opii*, *c. scammonii*, *c. sennæ*, etc., does not appear in the record: but, at all events, Israel, when sick, was sometimes drugged, which is contrary to the growing science of non-medication.

Medicine, or rather therapy, is not an absolutely exact science, but one of reasonable cumulative probability. It does not rest on the dictum of any one, but is founded on experience and observation. It is more reasonable to suppose that the general experience and observation upon which therapy rests are less likely to deceive, than the opinion, contrariwise, of a single or a few individuals. The testimony of the former must preponderate, other things being equal.

Therapy in no case can claim as its foundation a necessary truth; indeed, experience cannot prove a necessary truth, but probability only. It is not a pure science, like mathematics, astronomy, etc. Its materials, though consisting of facts, are related to and blended with vitality, or life, or antecedents, the nature of which have hitherto transcended experimental research. Those who deny the laws of vitality because they cannot define its cause, might deny as well many things altogether true. The nature of life, of mind, and even of matter, remain unknown. Physiologists have been no more puzzled to define life, than physicists to define matter. Hegel says that "a thing is nothing more nor less than the complex of its relations to other things. These relations are called its qualities; hence, the whole existence of a thing is *qualitative;* it is not primitive, mere *quantitative* being to which qualities are subsequently imparted." Now, on the contrary, "it is a fundamental proposition in the philosophy of Schelling, that all difference is *quantitative.*" Some of the greatest philosophers, including Bishop

Berkeley, maintain that the existence of matter has never been, nor can it be proven.

The efficacy of medication—active medication (for all medication is more or less active)—can in no way be invalidated by the improper and unnecessary administration of drugs, as to their kinds, doses, times, cases, etc. A man sinking from hemorrhage should not be bled, nor one collapsed from cholera be purged; nor should another, who has already eaten too much, for that reason eat still more. All arguments against the use derived from the abuse of drugs are fallacious and irrelevant. The true question is this: Is the skillful use of quinine or any other drug less likely to arrest and remove a disease—an intermittent fever, for example—than non-medication or Nature; or, to use the language of common sense, can the malady be sooner or more certainly or completely cured by letting it run its course without any interference? The question, then, at issue, is not whether medication is required in all cases, as no enlightened practitioner will hesitate to admit that it is not. Whether unskillful treatment is not more dangerous than the omission of all treatment—whether antiphlogistic or stimulant treatment should be preferred—are all foreign to the issue involved, which may be thus put: Cannot a physician who is thoroughly acquainted with the existing medical sciences, as well as with the observation and experience of the past—one who, during his education, has witnessed and studied the effects of therapeutic treatment, as now practised at the bedside—cannot he apply remedial agents, so as in all probability to cure a greater per centage of the sick than would be in all human probability cured by abandoning them to the natural course and tendency of their diseases; such, for instance, as intermittent fever, pleurisy, pneumonia, cholera morbus, scurvy, diarrhœa, dysentery, itch, syphilis, gonorrhœa, etc.?

If it were possible for an educated physician to be wholly ignorant of the therapeutical history of past experience, when called to treat his first case, he might not be able, upon the recovery of his patient, to say whether the treatment, even in a case of actual cure, had or had not contributed to the recovery. The testimony of the past, and a comparison with the

present, together with daily experience in such cases, will enable him to arrive at probability, if not absolute certainty, even in individual cases; and this probability is strengthened in proportion to the number of observations, and in many cases he may attain to a reasonable certainty, such as in most departments of science and business is deemed reliable for human belief and conduct.

In no isolated case, from Hippocrates to the present, can it be demonstrated beyond the possibility of a doubt, that a cure has ever been effected by any mode of medicinal treatment. Nor can it be demonstrated that the sun will rise to-morrow, and that some person will die during that day. Nevertheless, the evidence of analogy, or past experience and observation, give the utmost probability that both events will happen. That Cæsar, Napoleon I, or Washington lived, is less probable than that either of these anticipated events mentioned will fail. History has often been falsified, as experience has shown, but the rising of the sun has failed neither during our own experience nor that of our predecessors in all the historical period.

The efficacy of the medical treatment of disease is founded neither upon any certain knowledge of the nature of vitality, nor upon the mode of action by which a remedial agent removes pain and arrests the march of a malady. All that is known in the premises is empirical, that is, experimental. The same is true of the whole circle of the experimental sciences, with, however, this great difference, that throughout the whole realm of inorganic nature, there is a fixity or uniformity unknown to the vital or organic world. It may be, nay, it is probable, that uniformity reigns in the latter equally with the former; but unfortunately the antecedents, functions and nature of the living economy are, in relation to our senses and modes of cognition, neither known nor fixed with the certainty which appertains to inorganic or non-vital matter.

Observation and experience, however extensive these may be, are not knowledge. The latter is the act of a sound, comprehensive understanding, together with rational deduction from facts. Therapeutical facts are peculiarly liable to misin-

terpretation. A gentleman, a few days since, took the trouble to relate to me the great success of a deceased doctor (of this city) in curing hydrophobia. "Now it has been ascertained," says Dr. Oesterlen, "by the exact observation of hundreds of dogs which had been inoculated with the poison, or bitten by others which were mad, that even under the circumstances most favorable for the production of the disease, that is, where no means or so-called preservatives of any kind had been employed, scarcely sixty or seventy per cent. of the animals became rabid." Now, if these sixty or seventy cases in the hundred had been treated by any remedy, that remedy alone would obtain the credit of so great a success.

If the physician possessed any certain means of knowing that particular cases would eventuate in recovery without any advantages from treatment, a vast amount of medication would thereby be prevented. But neither the believers nor skeptics in physic can predict with absolute certainty such a result, in any case whatever. Prophets there are none. Probabilities there may be for or against the occurrence of one of the only two possible events in a given case.

The theoretical stand-points from which therapeutical methods are viewed, and the consequent contrarieties, real or apparent, in practice, afford skeptics with plausible, if not logical, arguments against medication. In illustration of this and some kindred positions, I beg leave to introduce a few paragraphs from Dr. Oesterlen's work on "Medical Logic" (Sydenham edit., p. 238, *et seq.*)

"If we bring to the bedside of the same patient a disciple of Brown or of Broussais, an empiric of the old, or one of the modern stamp, an adherent of the so-called Vienna anatomical, or of the Giessen chemical school, a nerve-pathologist or a blood-pathologist, each will recognize a different state of things. The opinion which each forms of fever, for instance, and similar aggregates of symptoms, of their casual connexion and dependence upon various local or general changes and conditions, and of these in their relations to each other, will be different from that of the others. Each of them, if he reflects upon it at all, will form a different notion of all that he

has been able to observe; he will arrange and combine the various phenomena in the patient after his own manner—*i. e.*, in accordance with his own point of view; and, if the same remedy be administered in a given case, the assertions and opinions of each concerning its effects will equally differ. For each has expected from it different services and modes of operation in accordance to his previously formed theory; he will, therefore, interpret what he has observed in the manner which best corresponds to his own views, and in the remedy employed will acknowledge only such effects as it has been his aim to produce.

"So long as Brown's system prevailed, many believed that digitalis produced an excitement of the heart and circulation. According to their view, the pulse was first accelerated and rendered fuller before it could become weaker and slower, and they asserted that the subsequent depression depended upon the previous excitement, (Saunders, Hutchinson, and others). If another, a disciple of Broussais, for instance, had observed exactly the same thing, his interpretation of it would have been entirely different, because he would not attach the same signification, or an equal importance to such excitement of the circulation, etc. In like manner, at the period when the simple, non-mercurial treatment of syphilis prevailed, patients were believed to be injured by mercury in a degree never observed before or since. A homœopathist of the old school, and a modern disciple of Rademacher, will equally claim for themselves, and for the influence and mode of operation of their remedies, credit for much which the impartial, and, perhaps, correctly reasoning physician, regards simply as dependent upon the natural course of the disease. The ordinary practitioner will scarcely hesitate to assert that he has obviated various diseases or abnormal conditions by means of some remedy or mode of treatment. He willingly regards the changes which take place in the state of his patients after the employment of them, as their effects, when those changes are favorable—*i. e.*, as the results of his practice; while, if the changes are unfavorable, he attributes them to the natural course of the disease, or to various other influences and circumstances. He will further interpret the effects of his remedies in

exact accordance with his own theories and views. Because, for instance, in persons submitted to the influence of anæsthetic agents, intelligence and consciousness, in the first place, then sensation, motion, and respiration, are successively suspended, ether and chloroform are said to act first upon the cerebrum, then upon the cerebellum; next upon the posterior, and, lastly, upon the anterior pyramids of the spinal cord and their nerves. And when their action is extended to the medulla oblongata, life is said to hang by a single thread. A Flourens, a Baudens, and others, take this view of their mode of operation, because it happens to correspond with their theories of the functions of those portions of the nervous system.

"In relation to the accuracy and scientific value of our conclusions, there is scarcely any difference to be found between the ordinary practitioner, or even the homœopath or disciple of Rademacher, and the modern physiological physician; for the endeavor of the latter towards a more scientific advance and judgment is still far from complete, and seldom, to any great extent, feasible. His interpretation of what he observes, though it may approach nearer to the actual state of things than theirs does, will not be less uncertain and arbitrary. Just so will the physicist interpret the same phenomena and processes in the living body, and even in the inorganic world—*e. g.*, in the air, water, or the soil, with their action upon each other, upon vegetation, man and animals—very differently from the chemist, and both again differently from the professed physiologist or ordinary practitioner.

"In all that has been already advanced, many may, doubtless, if they are so inclined, find sufficient grounds of discouragement, and feel justified in mistrusting even the first and most indispensable methods of comprehension in medical science. Yet, to perceive difficulties does not necessitate that we should allow them to overcome us; and in the case of observation, especially in such a science as Medicine, it has already been shown that it is by no means such a simple and intelligible thing as might be at first believed, and that Fontenelle was quite right when he

said 'l'art d'observer, que n'est que le fondement de la science, est lui-même une très-grande science.'

"If we compare, for instance, our present methods of observation and investigation in the whole field of vital phenomena, and even at the bedside, with those employed but a few decennia ago, we cannot fail to recognize a great improvement in them. Not only have we acquired, by the recognition of various anatomical relations and chemico-physical processes, better starting-points for our observations, but, by the employment of such aids, the general value of our observations is advanced far beyond that of earlier periods. In illustration of this it may be sufficient here to mention the more frequent and efficient employment of various auxiliary means, such as the thermometer, scales, etc."

The ethical question involved in medication or in non-medication is, in numerous points of view, of grave import. If opium, quinine, calomel, blistering, bleeding, etc., can either directly or indirectly remove disease, or even lessen pain, teachers, writers and practitioners who deny the efficacy and withhold the application of remedial agents might seem to plain people not altogether guiltless of hypocrisy, the omission of duty, and the propagation of fatal errors. Is it not hypocrisy to assume the office of a physician, which both virtually and openly holds forth to the public a belief in the efficacy of remedial agents, while at the same time the practitioner who receives compensation from patients for services wholly worthless, according to his own showing? Does he not obtain money under false pretences? Would he, in any case, be employed, if he candidly told the patient that all medicine is not only useless, but injurious, and that Nature only, not the doctor, is competent either to relieve or cure the sick? Can an honest man hold such opinions, and at the same time continue to practice his profession? Are not his principles and his conduct the antithesis of the true and the good?—the concentration of the false and immoral? If sincere in his belief, should he renounce practice altogether? His opinion, in opposition to that of a vast majority of his contemporaries and predecessors, who have had an experience in therapy to

which his own is as a drop to the ocean, may in all reason be disturbed; at least, until further discovery, he can never be certain that science and art can do nothing to arrest disease and prolong life.

If he can do nothing himself, how can he be certain that others cannot do something to arrest the disease? Can he fix the limits of the possible? Is he entitled to teach, write, or say anything more than that, for himself, he knows of no remedy which can in any case be useful for the sick, although others equally able to judge in the premises may know how to apply medicinal agents so as to cure? and, therefore, he cannot, from his own negative knowledge, infer that the positive knowledge of all others is to be rejected as useless, and, therefore, he cannot conscientiously propagate, by lectures, writings, or otherwise, a mere personal negative which might endanger the lives of millions, should medicinal agents, after all, be efficacious or curative.

Homœopathy, under the cloak of medication, when really carried into effect, is strictly non-medication. All its cures, if any, are effected by practising directly the reverse of its theory, of its infinitesimal doses, dilutions and attenuations. The expectant method of cure, a pleasant phrase or refinement in language, which means skepticism or non-medication—no more, no less, the practice of which (if the practice of nothing were a possibility at all conceivable) is but a deception, as far as the public is concerned, and would not be considered otherwise if candidly avowed at the bedside. Can any sane man suppose that a planter of Louisiana would send at midnight, in a rain storm and thunder, over rivers and bayous, through canebrakes, morasses, and among crocodiles, for a physician to visit his family, or his negro, if the sender knew that the doctor would not in any case administer any medicine, but leave the case wholly to what is fancifully called Nature? If the doctor mean by Nature, God, and that God is the medical attendant, he must be competent, even though he may not have a sheep-skin diploma. Would it not be presumptuous in such presence to prescribe pills, syrups and distilled waters, which the prescriber himself acknowledges to be wholly inert, and intended only to deceive?

Physiology.

ON THE CHRONOMETRY OF LIFE.

By Jas. Paget, Esq., F.R.S., Assistant Surgeon to St. Bartholomew's Hospital.

The design of this discourse is to illustrate the law that the processes of organic life are regulated with a regard to time, as exact as that which is observed by them in respect of size and weight and quantity of material employed by them; and to show that such an observance of time is characteristic of life, depending essentially on properties inherent in the living bodies themselves, and not on conditions external to them. Laws indicating the limitation of the organic formative processes, in respect of quantity, are evident in the facts that, in the ordinary conditions in which each living being is found, it and all its parts have appropriate size and weight and mutual proportion. These may, indeed, be modified by the variations of external conditions, or by events that are of the nature of accidents; but the range of possible variations is, in nearly all cases, comparatively narrow; and the boundaries are soon reached, in which changes of external conditions become incompatible with life. An instance of corresponding limitation of the organic processes in regard to time might be noted in the natural duration of each creature's life. It is, indeed, not possible to assign any exact number of hours, days or years, as the constant limit of life in any species; but it is enough to prove a law of time, as limiting the total duration of the organic processes in each, when we see that, in man, and in other species, the length of life, when not diminished by disease or violence, is as fixed as the natural weight or stature is, and that the term of life is marked by changes whose source is inherent in the living body. Watching these changes in the senile degenerations of the human body, it is evident that life does not cease, naturally, because of any change in the external conditions of living; and that the body is not, with advancing years, gradually worn out, as if there were a gradual consumption of a store of material or of force; but that, as, at a set time, the development of the body ceases and growth goes

on, and then growth ceases and the body is only maintained in its perfection, so, after a time of such maintenance, the method of the formative processes in the body changes, it slowly degenerates, and through degeneracy dies. And all these stages are alike natural, constant, timely; all, too, are together characteristic of life; there is no such succession of events to be traced in any form of dead matter. Observance of time may, again, be noted in the formative processes concerned in any of the organs whose changes mark the divisions of a life into its chief periods; *e. g.*, in the teeth. Considering merely the conditions in which the teeth of the first set are placed before they project from the gums, there appears no reason why one should be cut before the other, or why they should not all grow with equal speed. Yet while they all grow alike in regard of structure and composition, they have very different rules in regard to the time-rate of their formation. And a yet more marked instance of time-regulation is in the contrast of the teeth of the first set with those of the second. In all essential characters, except those of strength and size, the two sets are much alike; yet there is the widest difference in the rates at which they are formed, and in their duration. The second teeth require as many years for their formation as the first require months; the first live but a few years, the second should live as long as the rest of the body, and sometimes do so. Now there appears nothing to which, as to an efficient cause, this difference can be referred. Its utility and final cause can be discerned; but, as to that which verily determines the rates of growth, and the durations of the teeth, it can only be referred to a First Cause; or it may be said, as of other things subordinate to a First Cause, that it depends on some of those properties which each living being inherits from its parents, and through which it results that, in respect of time, as well as of method and quantity, the formative processes in the offspring are a repetition of those of the parent. The observation of the development and changes of the teeth affords, moreover, an excellent instance of the punctuality with which time-work is regulated in the organic processes, and of the manner in which several different, and really independent processes, being

set to the same time-rate, are made to coöperate to the end of utility in the economy. This is evident in the coincidence of the development of the teeth of the second set, with the removal of those of the first; and in the coincident growth of the jaw, and all its muscles and other apparatus for mastication. In all of these (and the same might be said of any other system of organs in any species) the formation of every part is achieved with an admeasurement of time as precise, and as perfectly designed as that of its shape, or size, or structure. For examples of organic processes, adjusted to be complete in definite periods of time, the germination of seeds and the hatching of eggs could be cited. In plants, and in cold-blooded animals, the time varies according to temperature, yet not without evidence of a proper time-rate; but among birds, each species has its own time for incubation, as fixed as its other specific characters. In other words, the development of the structures of an egg into those of a young bird, appropriately fitted for life in the open air, is timed to a certain rate of progress; so much work is to be done in so many days, neither more nor less; and on each day its appropriate and special portion of the work. And it is evident that the time occupied in the process is determined by the inherent properties of the egg itself. For if the eggs of any number of species be exposed to the same heat and other conditions, in a hatching machine, then, as surely as the bird produced from each will be like its parents, so surely will it be hatched in the same time as its parents were; in other words, the observance of a specific time-rate in the process of development is as exact as that of any other specific character. With this observance of time in the development of the young might be noticed that which is, commonly, coincident in the parent. Not to cite the example of all the mammalia, that of pigeons might be taken, in which, during the incubation of their eggs, the crops of the parents are remarkably developed, so that they may be fitted for the secretion of a fluid destined to make the food of their young offspring more suited for their sustenance. The correspondence of these time-rates, observed at once, in the development of the young pigeons, and in that of the crops of the parents, demon-

strates, in both, a provision for chronometry in their organic processes, as clearly as the faces of two clocks, constantly keeping time together, would prove that they both have some apparatus for chronometry within. Further, the provisions made by parents for their future young afford evidence of the time-regulation of organic processes, in so far as those provisions seem to indicate a reckoning of the time necessary for their completion. For example, certain turtles lay their eggs in hollows made in the sand, leave them there to be hatched, and at the time of hatching return to them for the sake of their young. It might be asked, how can these creatures, and many others in similar cases, reckon the passage of time? Most probably, they do not reckon it at all: but just as the timely-attained fitness of their organization for preparing and filling their nests impelled them to those acts, so some time-regulated organic processes, taking place in them after the laying of their eggs, bring about at length a new condition, of which a dim consciousness becomes an impulse to them to return to their nests. Such an explanation would involve little guess-work; for changed organization is, manifestly, often the source of impulse to instinctive actions, and the parental organization does commonly change at a rate commensurate with that of the development of the offspring. And a similar reference to chronometric processes in the body might explain many, though probably not all, other instances in which animals seem to have a power of reckoning the passage of time. The phenomena of disease, especially in fevers, agues, the consequences of injuries, and many cutaneous eruptions, would afford abundant instances of the observance of time in the organic processes. The vaccine disease might be generally watched as an illustration, being characterized by a vesicle at each place of insertion of the virus, which vesicle begins to appear on the third day, and on the following days passes through changes which are as exactly regulated in time as they are in visible characters. The changes in this vesicle are, moreover, indicative of a coincident succession of events in, or produced by, the virus inserted, which, in the blood of the vaccinated person, increases, and, incorporating itself in the vesicle, reaches its highest development

and greatest inoculating power on the eighth day, and then degenerates. The vaccine might, in most essential points, be regarded as a type of morbid poisons, *i. e.*, of such as are the products of disease. Whether inserted in the blood by inoculation or bred therein, they commonly occupy definite periods of time in their development, and increase, and decline; as with a life which is chronometric in all its phases and in its total length. The instances of morbid poisons would supply examples of organic processes timed to various numbers of days; and many that are completed in a day, or in given portions of a day, are traceable in the events of sleep and waking in animals (and, perhaps, also in plants), in the daily variations of the pulse, and of breathing, the returns of hunger and thirst, the regulated times of the digestive functions, &c. In man, indeed, consciousness and will are so concerned in some of these functions, that they may seem to lack that regularity which belongs to merely organic processes; but, if studied generally, and in other species as well as man, they all tell of such processes accomplished with regular measurement of time, and not determined by the external events or conditions of the day or night. Thus, for sleeping and waking, and the times of hunger and thirst, man's independence in regard to day and night, or light and darkness, and the habits of different species whose times of activity are severally, in the early or later day, in twilight or at night, may prove that the earth's diurnal changes are not the causes of these diurnal peculiarities of animal life. The very cause of sleep, and of that which is yet more mysterious, waking, may be unknown; but they are evidently connected and correlated with those alternating conditions of the structures, of which men, and probably all animals that sleep and wake, are conscious in the sensations of fatigue and of refreshment. The ordinary activities of one portion of the twenty-four hours, the activities, especially, of the muscles and nervous centres and the senses, produce an amount of structural, or chemical change, which is exactly repaired in rest during sleep. In other words, the organic processes for the repair of structures changed (as all structures are) by exercise, are adjusted to such a rate, that in general, and on an average, in the

time of sleep, they may completely restore the parts that are impaired in the activity of waking time. And so, of that replacement of substances in the several structures and in the blood, which is the purpose of feeding; the processes of digestion and of the several stages of assimilation are so timed, as to accord exactly with the times of daily taking. The most minute observances of time in organic processes might be noted in organs that have rhythmic motions, as in hearts and breathing muscles, ciliæ, the vacuoles of certain zoospores, as Volvox and Gonium. In the Croonian Lecture at the Royal Society, in 1857, the speaker had endeavored to prove that these and other rhythmic movements in plants, as well as animals, are due to corresponding time-regulated nutrition. He had expressed his belief that "rhythmic motion is an issue of rhythmic nutrition, *i. e.*, of a method of nutrition, in which the acting parts are, at certain periods, raised, with time-regulated progress, to a state of instability of composition, from which they then decline, and in their decline may change their shape and move with a definite velocity, or (as nervous centres) may discharge nerve-force." And this would be still maintained; but whether it were true or not, the rhythmical nutrition of rhythmically-acting muscles would be certain. If not a cause, it must be a consequence of such acting; for it is inconceivable that the heart (for example) or the diaphragm, or any other rhythmic muscle, should be free from waste or impairment in its action, or from the necessity of being renovated in its rest. Difference of mode of action could not determine a difference in the immediate effect of action. With long exercise, muscles become so changed that their changed state can be felt in the sensation of weariness, and proved by chemical analysis. But the change thus proved is only the accumulation of the changes wrought in many muscular actions, each of which has contributed a share to the whole amount, just as each revolution of a wheel contributes to the final wearing out. Similarly, every action of the heart, or of the breathing muscles, is attended with change or impairment of composition; but the impairment is repaired in the next following period of rest or relaxation. In other words, the alternating actions in

shortening, and rests in lengthening, of the muscular fibres are correlative and synchronous with their alternating impairments and repairs of composition. The chronometry of such organic processes seems perfect; nutrition is in them divided, as it were, into units; and for each unit there might be reckoned a unit of time.

Two results of this constant maintenance of rhythmic muscles are remarkable, viz.: the enormous power they are capable of exerting, and their freedom from fatigue when only naturally acting. The latter result is proved to depend on the constant maintenance of the muscles, in their timely intervals of rest, by the weariness which is produced in the same muscles when they act otherwise than ryhthmically, as in the muscles of respiration when employed in any voluntary movements, or in coughing, or other violent respiratory acts. The instances adduced thus far might supply examples of organic processes adjusted to periods of time varying from the length of human life to less than a second. They were all examples of large classes of facts, from which might be filled up the instances of observance of other and very diverse periods of time; and in all of them, the time-rate is essentially determined, not by external conditions (though these may, in some measure, modify it) but by the inherent properties of the organic bodies themselves. In another large group of instances, those, namely, in which vital processes are completed, or attain some climax, in a year or in a set portion or season of a year, an independence of external conditions appears less evident. The higher organisms, chiefly by reason of their having in themselves the power of generating heat, may manifest their own time-laws with comparatively little disturbance from without. But in the vegetable world, and in the lower animals, the organic processes are, for the most part, suspended during part of the year, for want, chiefly, of the heat which is a necessary condition of their activity, and the variations of which, for the rest of the year, very greatly affect their rate. Yet even in these there appear sufficient indications that the times in which the processes of organic life are accomplished depend, essentially, on the specific properties of the several organisms themselves.

Thus, under the same external conditions, each species observes a proper rate of its own. All the plants, for example, of a given locality are subject to the same temperature, and other seasonal conditions; but their rates of living, like those of various eggs placed in the same heat, are different; each reaches the chief events of its life at a certain period of the year. Variations of the seasons may affect all of them; but their method of succession is not thereby changed; they observe the same proportions in the times severally required for their organic processes; and this unaltering proportion indicates a time-rate specific for each, though equally variable in all. Moreover, among plants, there are numerous examples of varieties, which differ from the general character of their species only, or chiefly, in regard to the times at which their vital processes are accomplished. Such are the variations that are known as "late," and "early," among flowers or fruits; of which some may be propagated by seeds. [Specimens were shown from two-horse chestnuts growing opposite to one another by the great gate of the Kew Gardens, of which one is, every year, three weeks earlier than the other, in all the processes of its life; and of varieties of Erythronium, Dens Canis, from the same gardens, the plants of which, growing side by side in the same bed, always present a similar difference in their times of flowering, &c., though in all other respects alike.] It would be difficult to imagine a variety thus marked only by a peculiarity in rate of living, if temperature, or the influence of the seasons, alone determined the rate of life in the species. The simplest explanation seemed to be that, as there may be varieties in size and number of organs, and almost all the other properties of a species, which together make up its specific character, so there may also be varieties in regard to that time-rate of the processes of organic life which, even by this variability, is indicated as essentially dependent on the properties of the organism itself. Again, there are some species in which there seems to exist a singular independence of external conditions. Instances of this are found in the Eriogastor lanestris, and the other moths mentioned by Kirby and Spence. If pupæ, formed in June or July, be "selected of the same size, and ex-

posed to the same temperature, the greater number of them will disclose the perfect insect in the February following; some not till the February of the year ensuing, and the remainder not before the same month in the third year." (Vol. iii. p. 264.) The design of so singular an arrangement is, as they observe, to secure that insects, coming into active life in February or March, may not be utterly exterminated by the ungenial weather of a single season, or of two such seasons in succession; but the very cause of the differences among the pupæ, in their relations to the same external conditions, must be in their own properties. A somewhat similar instance of apparent complete likeness among seeds in all respects except that of time, is in those of a Begonia, which, if taken from the same pod, and all planted together, and all kept in the same conditions, will germinate, some in a day, some at the end of a year, and some at various intermediate times. To these indications of self-dependent time-rates in the lower organisms, might be added all the facts of another class, which show punctuality in the adjustment of several distinct processes. Scarcely an event of life could be watched which would not show it. [The instance by which it was illustrated was that of a Saxifraga, whose stamens, like those of Parnassia, arrive at their very maturity, not all together, but in pairs, and in pairs bend upon the pistil, each pair rising again before another pair bends down.] And, lastly, the influence of temperature on the rate of the formative processes in the lower organisms is scarcely, or not always, greater than that of nutriment and other external conditions is on their quantity. The occurrence of "good" and "bad" seasons indicates the latter influence, as that of "early" and "late" seasons does the former. Plants of the same species growing, some in an arid, others in a rich soil, differ exceedingly in size; the one are stunted, the others exuberant: here nutriment modifies the quantity of formation, as, in other instances, varieties of heat will modify its rate. But this being so, it may be held that as a certain average size or quantity of growth is a characteristic of each species, and an issue of its very nature, so is a certain average time or rate of growth. Quantity and rate may alike be varied by ex-

ternal circumstances, but the standard or medium of both, as well as the limits of variation compatible with life, are determined by the natural and inherent properties of the species. Whatever evidence these and the like facts might supply, that, in connexion with the seasons, the time-rates of the organic processes in the lower organisms are essentially dependent on the inherent properties of each organism, similar evidence might be adduced for the case of the higher, and especially the warm-blooded animals. In these the varieties of seasons have less influence in modifying the rate, as well as all the other measures, of life; and the less influence, the higher the species, or the degree of development of the individual. Moreover, there are in birds some instances in which organic processes have a tendency to observe certain times of the year even when the seasons are changed. Thus, among those brought from Australia to this country, some of the parakeets breed here in December; the black swan sometimes breeds in November as well as in May; the New Holland Cereopsis-goose has bred at the Zoölogical Gardens every February for five or six years. Among migratory birds, also, it has been observed that when they are kept in confinement, and removed from all the circumstances that might be supposed to induce or necessitate their journeys, they yet become restless at the return of the season for their migration. In these and the like facts there appear indications of a chronometry in the organic processes of warm-blooded animals, which corresponds with that of the seasons, but is essentially independent. And, if it be so, these might form a group of facts, in addition to those of the diurnal variations of the organic processes, in which vital changes are set to the same rules of time as changes of the surface of the earth, yet have their own proper laws; and concerning which it might be said, that the cycles of life, and of the earth, do indeed correspond, but only as concentric circles do, which are drawn round one centre, but are not connected, except in design and mutual fitness. But, however this might be, all the instances of time-regulation cited in the discourse (all being examples of large groups of facts), would seem sufficient to prove that the observance of time in organic

processes is as exact and as universal as that of any other measure: that each species has a certain time-rate for the processes of its life, variable, but not determined, by external conditions; and that the several phenomena commonly studied as the periodicities of organic life, are only prominent instances of the law which it was the object of the discourse to illustrate.

—*Med. Times and Gazette, June* 11, 1859, p. 593.

LECTURES ON EXPERIMENTAL PATHOLOGY AND OPERATIVE PHYSIOLOGY:

Delivered at the College of France during the Winter Session, 1859–'60.

BY M. CLAUDE BERNARD.

Member of the French Institute; Professor of General Physiology at the Academy of Sciences.

LECTURE XX.—EXPERIMENTAL PATHOLOGY ON THE ANALOGY BETWEEN THE ACTION OF MORBID CAUSES, AND THAT OF POISONS.

Summary: Reflections on the General Characteristics of Morbid Causes.—They require to be examined, like all other Agents introduced into the body, in their Physiological Agency.—Injustice of Reproaches usually addressed to Medical Men on their Ignorance of Biological Causes.—Certain Problems, in Science, can not possibly meet with a Solution, the Human Mind not being able to seize the intimate Nature of the Properties which belong to Matter.—Practical Results always connected, however, with the accessible Part of Scientific Questions.—Mode of Action of Morbid Causes compared with that of Poisons.—Woorara.—Strychnia.—Effect of Cold on the Motion of the Heart.—Singular Case of Poisoning, related by Corvisart.—Its Analogy with certain Forms of Disease.—In what Manner various Disorders ultimately produce Death.—Rational Autopsies.—They consist in examining successively all the Tissues, the Blood itself included.—The Liver taken as an Instance; contains no more Glycogenic Substance, towards the Close of fatal Diseases, in a variety of Cases.—Absolute Insufficiency of Post Mortem Examinations, as usually performed.—Practical Conclusions to be deduced from the preceding Reflections.—False Notions of the Power exerted over Nature by Man.—The Aurora Borealis which lately took place at Paris adduced as an Instance of this.—Necessity of combining Experimental Researches with Clinical Observations, and of comparing Healthy Tissues with Diseased Ones.—The Blood principally requires to be thus examined.—Chemical Analysis, in this respect, will never supersede Physiological Investigation.—Effects of Morbid Principles curable in a twofold Manner: 1st, by Neutralization; 2d, by Elimination.—The latter Process is the only one over which we possess any control.

Gentlemen—The general notions which, up to the present moment, have been the fundamental support of all our studies, are entirely derived from the parallel established at the very commencement of these lectures between Physiology and Pathology. We have shown you that physiology having, especially in these latter times, made rapid strides in advance of its elder sister, is now enabled to return the assistance which, at an earlier period, it received from the science of disease. We have, subsequently, examined the influence exerted by various agents upon the organs and tissues of the living body; a less extensive, and better

defined subject now lies open to our investigation; it is time that we should enter into the special domain of pathology, and commence the study of morbid causes. The method of inquiry which up to this point has been our guide, must still continue to direct our researches. To ascertain the mechanism of natural phenomena, and the mode of action of natural forces; to deduce from the knowledge of facts the practical consequences which belong to our subject—such must always be our aim, without attempting to solve the abstruse difficulties which lie beyond the pale of human knowledge. In our introductory lecture we exhibited the regular evolution which, step by step, carries a science from its very cradle up to the highest stage of development, and you have seen that in every possible branch of study the progress of the human mind is arrested at a given limit. "When we arrive at a certain point," says Lord Bacon, "Nature grows deaf, and answers no longer our inquiries."

The ignorance of medical men in these respects has often been the object of ironical reflections; and you well know the sarcastic reply with which Molliere answers the question: "Cur opium facit dormire?" "Quia inest in eo Virtus dormitiva." But in order to do justice to the sciences, and those who cultivate them, a question must never be put in such terms; each separate branch of natural philosophy might, in the same manner, be tried and condemned. If, for instance, it were inquired for what motive all ponderous bodies fall towards the centre of the earth, we should be equally unable to answer the question. The law which Newton established has taught us in what manner the force of gravitation acts, without throwing any light upon the intimate nature of that power. It is, therefore, as irrational as unjust to blame the biological sciences for a defect inherent to all human knowledge. Let us be satisfied with inquiring how diseases are produced, how medicines cure them, and how poisons destroy life. It has not been allotted to us to know why things are; but only in what manner they exist.

Our purpose is, therefore, to examine, in the natural order of succession, the phenomena which take place within the living body, when foreign substances have been ingested, in order to

learn the mode of action which enables it to produce baneful or salutary effects (as the case may be) throughout the whole system. We have found, for instance, that woorara paralyzes all the motor nerves, puts a stop to all motion, suspends the act of respiration, and thus brings on suffocation. The practical result is, that by artificially insufflating air into the lungs during a sufficient length of time, the animal is kept alive till the poison has been eliminated, and all the danger is past; now, the same takes place with all poisonous substances which do not produce actual disorganizations of the tissues; strychnia is in the same case as the former poison, for, if all the external excitement which perpetually provokes reflex action, and thereby brings on convulsions, which end in death,—if all these irritating causes, which inevitably act upon the animal left in the open air, are cautiously removed, all danger is avoided, and the animal slowly recovers, if the dose of poison has not been too large. Place a frog under a glass bell, in a cellar, after poisoning it with the alkaloid, if left there during a sufficient time, the animal is found to recover perfectly; while another frog, *cæteris paribus*, rapidly dies in violent convulsions, when repeatedly excited. The effects of cold may also be brought forward as an instance; when moderate, they benumb, but do not freeze the tissues; a frog's heart may be brought to stop under the influence of cold; but give a more favorable medium, and it will recommence beating; if frozen, however, there is an end of all vital properties, its tissue having been disorganized.

Such is the process of reasoning brought to bear upon physiological questions; an entirely similar method ought to be followed in clinical researches, for nothing resembles so much the action of poisons as that of morbid causes of an ordinary description: the analogy holds good even in those cases in which the disorder is wholly local, and does not end in death. Corvisart, in his celebrated work on Diseases of the Heart, relates the case of a young girl, who, in order to commit suicide, swallowed a very large dose of arsenic; some symptoms of poisoning followed, but the patient recovered, and, several months later, died of consumption. In making the autopsy, a large pseudo-mem-

branous cyst was found in the stomach; within this cyst was enclosed a solid mass of arsenic, which might have sufficed to poison nine or ten persons: this was the residue of the primitive dose, a very small portion of which had been absorbed, while the remaining part, incased in this accidental cavity, rested within the stomach without producing other effects than those of an ordinary and inoffensive tumor. Now, if the cyst had been a spontaneous morbid production, instead of resulting from the ingestion of a foreign substance, the consequences would have been identically the same, unless inflammation, suppuration and resorption had taken place within the tumor.

If we examine some of the diseases which most frequently produce death, we shall equally be obliged to have recourse to general effects, in order to explain the mechanism through which the ultimate result is attained. Numberless patients die of peritonitis, and in a very short space of time too: how does this take place? for peritonitis, at first sight, does not seem to interfere with any of the higher functions of life. Inflammation of the lungs, or pleura, frequently proves mortal in a few days; and in such diseases, the respiratory functions are of course impeded; yet mere asphyxia is evidently not the cause of death in acute cases of pneumonia; and, in affections which rapidly prove fatal, the animal, although deprived of food, cannot evidently be supposed to die from mere inanition in so short a space of time. It therefore becomes necessary to proceed to a rational investigation of *all* the diseased tissues, in order to ascertain the mechanism through which death has been produced: both nerves, muscles, glands, and other tissues; both the solids and liquids of the body require to be examined. If, for instance, the substance of the liver is submitted to chemical analysis, it is found to contain no more glycogenic principles; the total disappearance of which is, in our opinion, one of the most ordinary causes of death; for animals kept fasting for several days together still retain a certain amount of sugar in the blood. It therefore seems that life may be extinguished in two different ways: firstly, by the introduction of deleterious principles into the blood; and, secondly, by the total absence of indispensable elements in that

fluid. From such instances, it is hardly difficult to judge what degree of scientific accuracy we may expect to find, in ordinary post mortem examinations; local lesions are exclusively sought for, while the general disturbance passes unperceived; and, even supposing its existence to have been suspected during life, how difficult it becomes, in the human subject, to ascertain the fact after death! Twenty-four hours must have elapsed before we are allowed to touch the corpse: now, although in animals recently slain the natural properties of healthy tissues persist, during a certain space of time, we are perfectly aware that, after a few hours, they are no longer to be found: such, for instance, is the case with respect to the galvanic excitability of muscles and nerves in birds and mammiferous animals. If, therefore, the effects of woorara, digitalis, and other poisons, which act upon these very tissues, had been exclusively studied in the human species, we should never have been able to ascertain by comparison the precise nature of the injury.

To observe Nature in her operations is the only method of acquiring the power of turning them to our advantage. The extraordinary progress of science and industry in the course of the present century, and the astonishing forces which have, in consequence, been placed at our disposal, have given rise to several false notions, which, however, are at the present moment currently received; and we frequently hear it asserted that man holds the elements under his control; that fire, water, steam, electricity, and all the powers of Nature, obey his commands. Now, the exact reverse of these opinions is truth; and, as Bacon philosophically observes, "Naturæ non nisi parendo imperatur;" we are, in reality, only submitting to Nature at the very moment when she appears to obey our will. A singular phenomenon, of very unusual occurrence in our climate, has lately afforded us a striking instance of this. During the Aurora Borealis which took place at Paris in the course of last summer, strange and inexplicable perturbations were noticed in the action of the electric telegraph; the wires no longer conveyed the electric fluid to its ultimate destination, and for a short space of time, intelligence could no longer be communicated to Paris.

The impossibility of controlling Nature was in this case placed in a very strong light: had we been acquainted with the law which directed the unusual effects of that unknown cause, we might have obviated the inconvenience which our ignorance compelled us to undergo. The situation in which medical men are placed, in the treatment of diseases, is exactly the same; to alter their course is beyond our power, but the accurate observation of their successive symptoms sometimes enables us to interfere at the proper moment.

It is, therefore, altogether indispensable to combine experimental researches with clinical observation; to create artificial disease by known means in living animals, and proceed, immediately after death, to a rational post mortem examination; all the tissues must successively be compared to those in a normal condition, but the state of the blood more especially deserves our attention. Towards this subject the energies of all physiologists ought to be mainly directed. Organic chemistry, however, is unfortunately not in an advanced state, especially as regards the constituent principles of the animal organization; and chemical analysis must therefore be, in a great measure, left aside when the properties of the blood are the subject of investigation; in other terms, a physiological analysis is in this respect far preferable. Let the vital properties of the nervous system be brought into play, and modifications of the very highest importance will be discovered, which neither morbid anatomy nor the known methods of analysis would have been able to render apparent. The same is the case with muscles—when poisoned by certain substances, they lose their contractile power, but no modification whatever is to be discovered in their chemical constitution. The blood, therefore, may naturally be expected, in various states of health and disease, to present changes which none but the physiologist can appreciate; thus, when oxyde of carbon has been inhaled, the blood-globule is deprived of its characteristic property, of absorbing oxygen in exchange for carbonic acid; the fact is ascertained by physiological experiments, but ordinary analysis gives no clue whatever to the solution of the problem. Far be it from me to disapprove the chemical re-

searches of which the blood has been made the subject; on the contrary, they deserve to be carried out on a more extensive scale than ever; but if performed apart from experiments on the living animal, their results will never contribute to the real progress of science.

The study of morbid principles can, therefore, alone enable us to discover the means of curing the disorders to which they give rise; and for this purpose two different systems might be adopted; firstly, to neutralize the morbid agents; and, secondly, to eliminate them from the body. In the present state of our knowledge, we possess no means whatever of neutralizing their actions; as in the case of poisons, they must be expelled before they cease to act. To this result do all the efforts of Nature tend, and to this result also must all the physician's endeavors be devoted.

LECTURE XXI.—OPERATIVE PHYSIOLOGY—ON THE GASTRIC JUICE.

Summary: The Operation of Gastronomy, in Dogs, usually not attended with any serious Inconvenience.—Peculiar Properties of the Gastric Fluid.—Its Acid Reaction.—This Property persists throughout the Entire Scale of Being.—Utility derived from the Knowledge of the Fact by Comparative Anatomists.—Glandular Elements which secrete the Gastric Juice.—Not collected into a Conglomerate Body.—The Tube-Cells contained in the Mucous Membrane of the Stomach, are the Real Source of this Fluid.—Experimental Proofs of the Fact. Prévost and Leroyer's Experiments.—The same Results obtained in other Herbivorous Animals.—The Pyloric Portion of the Stomach alone concurs in this Secretion.—The Production of Gastric Juice is Intermittent, and only takes place during the Process of Digestion.—Remarks made by Dr. Beaumont on his Canadian Patient.—Experiments made upon Animals lead to the same Result.—Anatomical Modifications which occur for Notice in the Mucous Membrane of the Stomach when Digestion takes place.—The Epithelium falls off.—Various Substances eliminated by the Gastric Secretion, Prussiate of Potash, Salts of Iron, etc., etc.—Experimental Demonstration of the Fact.—Difficulty of Ascertaining the particular Point in which the Secretion takes place.—Intermittent Character of the Secretion, even in Animals, the Stomach of which is always full.—Chemical Reactions of Urine during Abstinence and Digestion, in Herbivorous and Carnivorous Animals.—Elimination of Urea, and Salts of Ammonia by the Gastric Cells, when the Kidneys have been removed.

GENTLEMEN—The various methods of obtaining gastric juice in large quantities having been fully described in our preceding Lectures, we shall now proceed to examine the principal properties of this important fluid.

The animal, into the stomach of which an opening was made last week, died shortly after the experiment, in consequence of peritonitis; but this unfortunate result must not be attributed to the operation itself, which, in most cases, is not attended with

any serious inconvenience, but rather to the animal's previous state of health, and those unfavorable predispositions too often observed in certain breeds of dogs, with respect to operations performed upon the abdominal organs.

The experiment has been successfully performed in the animal which we now place before you; it appears, as you perceive, to enjoy perfect health for the moment, and will furnish us, when necessary, with an abundant supply of gastric juice. Let us now, therefore, proceed to examine the characteristic properties of this secretion.

Although its chemical composition slightly differs in various animals, the gastric juice is constantly endowed with a strong acid reaction; a fact established, towards the beginning of the last century, by the ingenious experiments of Réaumur. This property of the secretion persists throughout the entire scale of being, and comparative anatomists have often been enabled, in consequence, to determine the precise seat of the stomach, in doubtful cases, the reaction of all the other parts of the digestive apparatus being neutral, or even decided by alkaline; the use of colored tests enables therefore the observer to point out, with perfect accuracy, that portion of the intestinal tube which corresponds to the animal's stomach; in certain fishes, for instance, no modification whatever in the shape or size of the alimentary canal corresponds to the place which the organ occupies; so that without the use of colored tests, to discover its position would be altogether impossible.

But the glandular cells, which pour into the cavity of the stomach this powerful solvent, are not collected into a conglomerate mass, but disseminated over a large surface. In former times, the inventive powers of anatomists had been strongly tasked, for the purpose of discovering the organ which produces the gastric juice; and, among other viscera, the spleen had been invested with this property; but it is now universally acknowledged that the small tubular glands, which the microscope exhibits in the mucous membrane of the stomach, are, in reality, the only source from which this fluid is derived. But how is this to be experimentally demonstrated? The secretion of these

little bodies cannot, as in the case of large conglomerate glands, be collected by means of a tube introduced into the excretory duct, and afterwards analyzed. Now the mucous membrane, which contains them, possesses, at the same time, a considerable number of small mucous follicles, the structure and properties of which are entirely different from those of the abovementioned glandulæ; direct experiments are, therefore, evidently required to settle the question. The first satisfactory solution of this difficulty is due to Prévost and Leroyer, of Geneva. These gentlemen have proved, by the following experiments, that the pyloric portion of the stomach alone enjoys the property of secreting the gastric juice. The stomach is opened in a living animal by an incision made upon its anterior surface, the abdominal walls having been previously divided; its inner surface is then gently wiped dry with a fine sponge, imbibed with a weak alkaline solution, care being taken, of course, not to rub off the epithelium; lastly, blue litmus paper is introduced into the cavity, the wound is closed, and after the lapse of a few hours the animal is killed. It will then be found, in making the autopsy, that, towards the pyloric extremity of the organ, the blue test paper has turned red, while in the other portions of the cavity it has retained its primitive color, a result which proves that the pyloric portion of the stomach containing the little tubular cells previously alluded to, is the real seat of the gastric secretion.

The experiment may be repeated on various kinds of animals, and invariably leads to a similar conclusion: thus in the horse, the pyloric portion of the stomach, and in the Ruminantia, the *red*, or pyloric portion of the apparatus, are alone endowed with the property of secreting the fluid, as the colored tests sufficiently evince. We may therefore consider it as an incontrovertible truth that towards the pylorus alone is the gastric juice produced.

But the secretion of these little glands is an intermittent one, in man as well as in the lower animals. Dr. Beaumont states that, in the case of his Canadian patient, the fluid only made its appearance when food was introduced into the stomach, but as soon as the digestive process was over, no secretion whatever

took place within the gastric cavity until the patient had made another meal. In the same manner, when a canula has been introduced into a dog's stomach, not a drop of liquid escapes, provided the animal has been previously kept fasting; but, as soon as it begins to feed, large quantities of gastric juice are at once seen to flow from the outer orifice of the tube; and the result may be obtained by merely showing the animal its food, as in the case of the salivary glands. In the dog which we exhibit here, the stomach is at present in a state of rest; but the sight of food will immediately stimulate the physiological activity of its glandular elements.

[The experiment is performed, and perfectly succeeds; a piece of meat being brought in, the gastric juice immediately flows in abundance from the open tube, before the animal has begun to feed.]

You perceive, gentlemen, that as in the case of most other glands, the process of secretion is not continually going on in the stomach. During the interval which separates one meal from another, the mucous coat is lined with a thick layer of grayish epithelium, and its reaction is alkaline; but as soon as the digestive process commences, it swells, becomes ingested, and grows red; the epithelium falls off in scales, and the gastric juice appears on the inner surface of the stomach, in the same manner as perspiration on the skin. It seems probable that some similar process takes place in all glands during the period of rest. Saliva, for instance, when it begins to flow, contains a considerable proportion of epithelium at first; but the liquid soon becomes transparent, and contains only a few epithelial cells.

There exists a remarkable difference between the gastric juice and these secretions we have already studied, with respect to the substances eliminated from the economy by the respective glands, after having been introduced into the vessels. We have shown you, for instance, that prussiate of potash and the salts of iron, in general, were not admitted into the salivary secretion; now, the reverse takes place in the case of gastric juice. When prussiate of potash is injected into an animal's veins, if one of the salts of iron is introduced into the stomach, the mucous mem-

brane soon assumes a deep blue color in its pyloric portions, while digestion is taking place: the two substances having been brought into contact at the orifices of the tubular glands which secrete the digestive fluid, Prussian blue has been formed on the very spot. The same result may be obtained, but with much greater difficulty, by reversing the experiment, and injecting the salts of iron into the veins, while the prussiate of potash is taken by the mouth; and, lastly, when these two substances are separately injected into the vessels, the same effects are still observed; the reaction does not take place as long as they are contained in the blood, but is produced immediately after they have been set at liberty by the process of elimination.

In making the above-mentioned experiment, I had entertained the hope of discovering the precise seat of the gastric secretion: the two substances being introduced into the circulation on distant points, I had expected to find the Prussian blue which results from their combination contained in the very glands which separate them from the blood; but, in this respect, my expectations have not been realized; for on examining, under the microscope, the mucous membrane in which the reaction had taken place, I found that the ferro-cyanuret of iron was deposited on its surface, and not within the cavity itself of the secreting tubes.

We have just stated that the secretion is an intermittent one, and that this is the case in all animals; but how can the fact be proved in rabbits, and other Rodentia, the stomach of which is never empty? The inspection of the urine allows us, by an indirect method, to arrive at this conclusion; for, in all herbivorous animals, this fluid offers, during the process of digestion, an alkaline reaction, and becomes acid as soon as the process is over: in the Carnivora, the reverse takes place; and this difference results merely from the nature of their food; for dogs exclusively fed upon potatoes and other amylaceous substances acquire in this respect the properties of herbivorous animals; their urine grows acid while the digestive organs are at rest, and alkaline as soon as they enter into an active state; while in rabbits, exclusively fed upon meat, the urine becomes, on the contrary, entirely similar to that of the carnivora. If, therefore, one

of these animals is examined, when fed as usual upon vegetables, it will be found that, after a long period of abstinence (twenty-four hours, for instance), the urine becomes quite acid, we are therefore entitled to believe that digestion has been awhile suspended, although the mucous coat of the stomach remained perpetually in contact with portions of undigested food. But in certain cases of disease, the natural conditions of the gastric secretion being altogether perverted, an uninterrupted flow of liquid takes place. We have informed you, in a previous part of this course, that the stomach often eliminates certain bodies, which for various reasons can no longer escape through their usual channels. Prévost and Dúmas ascertained, many years ago, that when both kidneys were removed, in a dog, several days would elapse before it was possible to prove the existence of urea within the blood by means of chemical tests: another, and a different mode of elimination had therefore been provided for this substance; but these gentlemen had made no attempt whatever to give an explanation of the fact, when I discovered unequivocal signs of the presence of urea and salts of ammonia in the gastric juice of dogs which had recently been deprived of both kidneys. It therefore appears, that in this part of the body does the elimination take place during a certain lapse of time; and the animal's appetite does not diminish under these circumstances, nor is digestion impeded, but when the stomach can no longer perform its office in this respect, the obnoxious substances pass into the blood, and the symptoms of uræmia make at last their appearance. —*London Med. Times and Gazette.*

Excoriated Nipples.—Dr. Pierce observes that he has seen the stramonium ointment, as well as the whole list of astringent and stimulating preparations used; but not one has been so universally followed by good results as that made after the following prescription: ℞—Acid tannic. gr. xx., glycerinæ, alcohol. aa, ℥ j. M.—*Boston Journal.*

—*Med. Times and Gazette, Feb.* 29, 1860, p. 197.

Surgery.

ON PIROGOFF'S OPERATION.

By Dr. Eben Watson, Lecturer on Physiology in Anderson's University, Glasgow, and Surgeon to the Glasgow Infirmary.

[James M'G——, aged fourteen, received a severe injury to the foot whilst cleaning machinery. The whole foot, with the exception of the heel, was completely "smashed," as it is familiarly but expressively denominated.]

There could be no difference of opinion as to the propriety of amputation in such a case. The only question was where it should be performed. The parts were much too severely and extensively injured to admit of Chopart's amputation through the tarsus being practised with any hope of success. I might have performed Mr. Syme's amputation through the ankle-joint; but, from the shattered state of the foot, the dissection of the heel-flap would, I think, have been difficult. It seemed to me that this was a favorable case for performing the amputation recommended by M. Pirogoff; for the skin and bone of the heel seemed to have been uninjured, while no other portion of the foot was in a condition to be saved. My colleagues agreed with me in this opinion, and, therefore, the patient having been put under chloroform, I proceeded with the operation as follows:

With a straight, pointed, and strong bistoury I divided all the tissues down to the os calcis, from a few lines in front of the inner malleolus, to the same point on the outside of the ankle, and I connected the extremities of this incision by another across the front of the ankle. I then opened the joint, and cut the lateral ligaments carefully, especially the inner one, keeping close to the bone, that I might not injure the posterior tibial artery. I next sawed through the os calcis, from above downwards, close to the posterior margin of the astragalus; and, having cleared the articular ends of the tibia and fibula, I sawed off a thin film of bone from them, of course taking away with it the two malleoli. Some of the tendons that had escaped the knife were now shortened, and then the plantar arteries and the anterior tibial were tied. I now found

that when I brought up the cut surface of the heel bone to the cut surface of the tibia and fibula, they were easily placed in exact apposition, and were as easily retained there by three silver sutures in the front of the stump, bringing the ligatures out by the sides of the wound, which were left open, so as to admit of a free discharge. The stump was surrounded by wet lint enveloped in oiled silk paper, and when the boy had been replaced in bed, it was laid on a pillow. After recovering from the chloroform, he took twenty drops of laudanum, and slept pretty well during the night. For a few days he was feverish, and required low diet, laxatives, and even an antimonial mixture; but this passed off when suppuration became established in the wound. His health was at no other time affected during his stay in the hospital.

Locally, the treatment consisted in having the leg bandaged pretty firmly in a hollow posterior splint of pasteboard padded with cotton, and cut away at each side of the heel, so as to allow the discharge to run away freely. The wound was kept clean by changing the dressings every day, but the splint was only changed on the second, third, or fourth day, as it seemed to require. The ligatures came away on the sixth or seventh day after the operation, but the sutures were allowed to remain in twice as long. On their being removed, a strip of plaster was placed so as to keep the wound together, but I do not think it was either very effective or very necessary, and it was soon dispensed with. The posterior splint, and a turn of bandage brought round the point of the stump as well as laterally, were, I think, the means of keeping the parts in due apposition during the healing process. Water dressings first, and afterwards lint dipped in olive oil, were the only other applications used.

About three weeks after the operation, a small abscess formed above the inner ankle. It was freely opened, and healed readily. The original wound, too, had united healthily by the beginning of October, and the boy was then allowed to walk about on crutches, still, however, wearing the splint for the sake of greater security against injury to the stump. About the same time I observed that the calcis had become united to

the tibia and fibula, so as to be nearly immoveable. I did not, however, allow this to be very severely tested.

On the 17th of December, I showed this boy to the clinical class at the Royal Infirmary. He could then walk with perfect freedom on the stump, and without any lameness. As he stood before the class, there was no apparent deformity; but when the limbs were compared from the knees downwards, the left (or one operated on) was about half an inch shorter than the other. The stump was as perfect as can be imagined. It was difficult to make out the cicatrix in front. There was complete osseous union between the bones involved in the operation, so that they formed an united support for the body; and the skin of the heel, tough, strong and smooth, formed a very hardy covering for its extremity.

In regard to the performance of Pirogoff's operation, I would offer two remarks:—

1st. It is much easier to avoid cutting the posterior tibial artery behind the inner malleolus in this operation, than in that proposed by Mr. Syme; for every one who has performed the latter knows that he is not the least likely to cut the artery so long as he is attending to it—viz., while he is separating it from its attachments. It is when he is doing something else, and especially when he is dissecting back the skin of the heel, that his knife is apt to slip upon the vessel before he is aware. Now, in Pirogoff's operation, after the artery has been detached along with the skin below the inner malleolus, the operator cuts no more in that direction at all. He has only to cut the internal lateral ligament of the ankle-joint, keeping his knife close to and parallel with the astragalus, and then to use the saw; the soft parts being held back for him by his assistant, by means of a blunt hook, or with his fingers.

2d. M. Pirogoff, in his memoir describing his new operation, writes as follows:—"I separate the short anterior flap from the two malleoli, and saw through them at the same time close to their base." And again: "I turn this flap (the posterior) forwards, and bring the cut surface of the os calcis *in apposition* with the *articular surface* of the tibia. If *the latter be dis-*

eased, it is sometimes necessary also to saw off from it a thin slice with the malleoli."

I wish to draw your attention particularly to these directions, because I venture to differ from M. Pirogoff in regard to them. I even think that the unfortunate issue of some of his cases may be attributed to his following that plan of operating. In my opinion, you ought always to saw off the articular extremities of the bones of the leg; for then you have a clean surface of cancellar bone on either hand—viz., at the anterior part of the os calcis, and at the inferior extremities of the tibia and fibula. Such surfaces are the best adapted for speedy osseous union; whereas, if the articular surface of the tibia is left untouched, as I understand M. Pirogoff recommends to be always done when it is not diseased, the synovial membrane and cartilage must inflame and suppurate, and be partly absorbed, partly discharged, before osseous union can take place between the tibia and calcaneum. The position of matters is very different when soft parts are to be applied to the surface of a joint, as in amputation through the wrist-joint, which you saw me perform about three weeks ago, and which has healed without one of those untoward circumstances that used to be dreaded by surgeons in such cases. If, however, the operation of M. Pirogoff be performed without excising the articular surface of the tibia, we should have a state of matters more analogous to the excision of one surface of a joint, which few surgeons would recommend. The bringing up of the os calcis, and not a soft flap, upon the articular surface of the tibia, makes the greatest differences in the processes pursued by nature before healing is permitted. In the one case it is generally simple adhesive inflammation; in the other, it is, as I have stated above, a more lengthened and complicated, and therefore more dangerous process. Hence it is that I should recommend the surgeon in all cases to saw off a thin layer of the articular surface of the tibia along with both malleoli, before he brings up his posterior flap.

But if this be done, it may be asked why disarticulate at all? In thinking of this question, I planned the following procedure, which, I think, will be found easier than M. Piro-

goff's. The leg is placed on its side, the operator holding the front part of the foot to be amputated in his left hand. He then makes an incision with a bistoury across the side of the foot, from the tip of one malleolus to that of the other, carrying it right down to the os calcis. He then applies the serrated edge of a small amputating saw in the wound, so as to divide the os calcis at such an angle as will enable him to avoid touching the malleoli. The assistant ought to steady the os calcis by grasping the heel between his finger and thumb, while the operator is using the saw. The latter then resumes his knife, and, placing it between the divided surfaces of the bone, cuts a little upwards, till he gets fairly behind the upper portion of the ankle-joint. The posterior flap is now formed, and should be turned upwards on the back of the leg by the assistant, so as to keep it out of the way. The knife should next be carried in a circular manner round the anterior aspect of the joint, dividing the skin in such a way as to unite the points of the former incision by this transverse one in front. The skin in front should then be pulled up a little, and the tendons and other structures should be divided down to the tibia and fibula, just above the ankle-joint. Lastly, these bones are sawn through in a slanting manner, by directing the saw from before backwards and downwards. The posterior flap is now brought up, and it will be found that the cut surfaces of the tibia and fibula on the one hand, and of the os calcis on the other, will fit each other exactly. The skin in front is united by wire sutures, and the operation is finished.

I am not at all anxious to claim originality in regard to this modification. My aim is not to rob M. Pirogoff of any share of that honor which is justly his due, but to assist in perfecting and establishing his operation amongst the resources of surgery. Nor am I singular in thinking that the operation, as proposed by him, admits of improvement. Many surgeons in this country and on the Continent have suggested variations in its performance, and I only ask that the above method of operating, *without disarticulating*, which is its sole distinctive feature, may be carefully considered, as I have no doubt the other proposals have been.

I may remark, that I do not think the slant-cutting of the os calcis an improvement in itself, though it has been proposed by M. Sédillot so long ago as 1855, and again by Mr. Busk, of the Seaman's Hospital, in 1858. I have merely adopted it in my modification for the purpose of avoiding collision with the malleoli in sawing through the os calcis; but I believe that the less slanting the longer will be the limb, and the greater the ease of keeping the ends of the bone in apposition.

The method which I have proposed occupies less time than that of M. Pirogoff; the risk to the posterior tibial artery in disarticulating the foot, and the trouble of the additional dissection are avoided, while an equally good stump is made in the end. Besides, it will be found that, in performing M. Pirogoff's operation for some injuries of the foot, in which the greater part of it has been destroyed or lacerated, one of the surgeon's chief difficulties will be, the want of purchase in steadying the foot while he is sawing through the calcaneum after disarticulation. He can only hold it by the broken and lacerated front part; whereas, if he applies the saw in the way I have proposed, the attachments of the ankle-joint, and the possibility of the assistant's seizing the projecting part of the heel, make this work much easier. Again, in sawing off the articular ends of the tibia and fibula, he has the astragalus to hold by, instead of the slippery ends of the malleoli.

After I had performed this operation twice on the dead subject, and was convinced of its suitableness for the accomplishment of the object in view, I employed it in the following case:

Thomas M'C., aged thirty-three, carter; admitted on the 4th of January, 1859. "This afternoon a loaded railway wagon knocked the patient down, and passed over his right foot, producing great laceration of the soft parts, and fracture of many of the bones of the foot. The tissues on both sides of the ankle and foot are very much separated from the bones. Patient does not labor under any shock."

At a quarter past nine P. M., a consultation was called on this man's case. I then found his foot completely smashed except the heel, and, as stated above, the skin and soft parts were sep-

arated back to the very malleoli. Indeed, so bad was the laceration, that some of my colleagues recommended amputation at the lower third of the leg. On careful examination, however, I found that I could perform the operation above described; and I accordingly did so, making as good a stump as in the former case, with much greater ease and expedition. The patient was feverish for a few days after the operation, and received gentle antiphlogistic treatment. He afterwards progressed slowly but uninterruptedly in his amendment. Just as in the preceding case, the limb was placed in a posterior splint of pasteboard, the stump was covered with water dressings, and the whole was supported by a bandage. The dressings were, of course, changed from time to time, but no adhesive plaster was applied to the wound.

He was dismisssd, cured, on the 12th of March. His stump was sound, and the union of the bones perfect. He had been for some days accustomed to walk on crutches.

M. Pirogoff seems to dread the occurrence of two evils after this operation. These are—

1st. The death of the os calcis. Now, I can hardly think that there is any very great danger of this occurrence, especially if the posterior tibial artery is not divided too high up. And even if the performance of the operation is thus marred, still the branches from the posterior peroneal artery to the outside of the calcaneum would, I think, be sufficient to maintain its vitality. At all events, there is no greater risk of death of the os calcis than of the posterior flap in Mr. Syme's operation. The same precautions are requisite in both cases, and will be equally efficacious in both.

2d. Abscesses in the sheaths of the tendons are greatly feared by M. Pirogoff; and it cannot be denied that they are likely to occur in some of these cases. He recommends that the tendons be not cut too short in the formation of the flaps; otherwise, when the muscles contract, the sheaths will be left empty towards the wound, and, in his experience, more liable to suppuration. It is very proper to attend to this advice, but surely it is seldom that these abscesses, supposing them to have occurred,

are so very dangerous as he represents. The abscess is in most cases limited by exudative matter to a small part of the sheath, and, if freely opened when pus has formed, it generally proceeds no further, but heals kindly and readily. Such an abscess formed in the first of the cases which I have reported above, and it hardly retarded the progress of the case for a single day. It will, moreover, be obvious that this is a danger which is apt to present itself in all cases of amputation through parts supplied with long tendons, as at the ankle or in the forearm, but it has never been considered so very formidable by other surgeons.

It has occurred to me, that the splint, which I kept steadily applied in these cases, may have operated favorably in preventing the formation of abscesses in the sheaths of the tendons. This apparatus, no doubt, kept the whole limb quiet, permitted no jerking of the muscles, and prevented, to a certain extent, their contracting and pulling the tendons up from the cut extremities of the sheaths.

The chief advantages of M. Pirogoff's operation are—1st, that the length of the limbs is preserved as nearly equal as possible under the circumstances. M. Pirogoff's own statement is thoroughly borne out by my experience of his operation:—"The leg," says he, "after my operation, appears an inch and a half (sometimes more) longer than in the three operations (Syme, Baudens, Roux), because the remnant of the os calcis left in the flap, as it unites with the inferior extremities of the tibia and fibula, lengthens them by an inch and a half." In the case of the boy M'G., the left leg is only two-fifths of an inch shorter than its uninjured fellow; and in M'C.'s case it is not more than an inch and a half. This great difference, in these two cases, is easily accounted for—(1.) Everybody knows that there is great inequality in the length of the os calcis in different persons; some are more spur-heeled than others, and these persons, however clumsy their feet may have been before, would obviously make the best subjects for Pirogoff's operation. (2.) In M'C.'s case, the soft parts round the ankle were much lacerated, as formerly stated; and, in paring my anterior flap, I had to cut rather higher up than was desirable. The saw was also applied

fully high up, so that a good half inch of the tibia was cut off. Had it not been for this accidental circumstance, his limb might have been nearly half an inch longer.

2d. The skin and alveolar tissue of the heel are stronger and sounder in the stump after Pirogoff's operation than they could be if dissected off the heel, and applied to the ends of the bones of the leg, as must be done in any form of amputation through the ankle-joint. The support for the body is thus much better in the former than in the latter case, and the patient is sooner able to use it in walking. The boy M'G., on whom I first operated, began to walk on his stump as early as six or eight weeks after the operation, and, in less than four months after its date, he could use it with perfect freedom. He still continues to do so, and his defect is hardly observable, whether in walking or standing, although he wears a very clumsy artificial foot. The other patient, M'C., was, for some time, timid in using his stump; but by the beginning of May, when he last showed himself at the hospital, he had quite overcome that feeling. He had obtained a very good light artificial foot, of such simple construction, that it only cost a guinea. He could walk without a stick, and it was remarked by every one who saw him, that no stranger could discover from his manner of walking that he had lost his foot.

In conclusion, I think that M. Pirogoff's operation is a great improvement in surgery, and I am astonished to learn that he has himself departed from it; for so it was reported officially by Messrs. Mouatt and Wyatt to Sir John Hall (Fergusson's Surgery, 4th edit., p. 487). For my part, not only can I see no good reason for abandoning the operation, but I think its proposer deserves much credit. It seems to me preferable to any other form of amputation at the ankle-joint when the heel is sound; and I shall even go further, and maintain that it is, *in some cases*, preferable to Chopart's amputation through the tarsus. I refer to cases of injury of the foot, in which the latter operation is sometimes attempted, though it may be impossible, owing to the laceration, to procure a sound covering of soft parts for the astragalus. Now, when this is not done, the face of the

stump is apt either not to close at all for a long time, or to ulcerate whenever an attempt is made to use it. It thus remains painful and useless for a length of time, during which the muscles of the calf of the leg contract and pull up the heel, thus increasing the mischief both as to pain in walking and deformity of the injured limb. Illustrations of these remarks must have occurred in the experience of every practical surgeon, and two cases lately came under observation in our own hospital; the patients have sought relief because of their painful and ulcerated, and therefore useless, stumps. In the stump which remains after Pirogoff's operation, such a state of matters could never occur, both from its shape and from the fact that the operation wound is *fixed* high up in front, where it is in no danger of being hurt in walking.

I have said nothing as yet about the choice of cases for this operation, and, indeed, very little need be said at all. It is obvious that the heel bone must be sound, otherwise the case is not suited for Pirogoff's operation. When the tarsal bones are diseased, the os calcis is seldom free from the morbid affection; hence, in such cases, Syme's operation is generally more applicable; whereas, in accidental injuries of the front part of the foot, if neither Hey's nor Chopart's operation can be performed with a good covering of soft parts in front, then an admirable stump may generally be procured by adopting the procedure of M. Pirogoff. *Lancet, June* 11, 1859, p. 577.

Fractures.—*Pasteboard Splints.*—(By Mr. Acton, M. Merchie, p. 132.)—Pasteboard, or papier machie, may be manufactured into exceedingly light, firm and inexpensive splints. When wet, the substance may be accurately moulded to the part, and when dry, will retain the form so impressed upon it. A little cotton wool should intervene between the skin and the splints, (which should be divided into two equal halves) and the whole then enveloped into a bandage. This kind of splint was extensively used by the Russian surgeons at Sebastopol.

Midwifery.

LECTURE ON PARTURIENT HEMORRHAGE.

Being the Fifth of a Course on the Complications and Sequelæ of Labor. Delivered in the University Medical College, New York.

By T. Gaillard Thomas, M.D., Physician to Bellevue Hospital.

Gentlemen—There are three distinct periods at which the child-bearing woman is liable to an inordinate loss of blood, namely, during pregnancy, during labor, and for one month subquent to that process.

This division is by no means an arbitrary one, but is demanded by the circumstances of the case, and required for convenience of study and lucidness of understanding. Even the limit of one month given to the third variety is based upon good grounds; for, at the end of that time, the heretofore hypertrophied uterus having undergone involution so far as to have arrived at nearly its non-pregnant state, any flow taking place thereafter is properly regarded as disconnected with the puerperal condition.

These three distinct periods divide puerperal hemorrhage into three equally distinct varieties, viz.:

Hemorrhage before labor;

Hemorrhage during labor;

Hemorrhage after labor;

or, as they may be styled for convenience of reference,

*Ante-partum hemorrhage;

Parturient "

Post-partum "

The nature of the course which now occupies us will preclude the investigation of the first of these varieties, and we will proceed at once to that of the second.

Parturient hemorrhage includes every excessive flow occurring during the act of parturition, whatever be its source, its violence, or its results.

*The strict meaning of "ante-partum" is "before having brought forth," and a more exact term would be "ante-parturient," but the connexion will prevent any confusion, and the substitution of a new for an old familiar name is not desirable.

SOURCES OF PARTURIENT HEMORRHAGE.

The sources from which this hemorrhage may occur are these:

(*a*) The ruptured vessels of the os and cervix uteri;
(*b*) " " body of the uterus;
(*c*) " " umbilical cord;
(*d*) " " vulva;
(*e*) " " which unite the uterus and placenta.

(*a*) As the os and cervix uteri dilate in the first stage of labor, the arterioles which thickly stud the mucous membrane generally rupture, a small amount of blood pours forth, mingles with the tenacious secretion of the glands of Naboth, and constitutes what has been called, in the language of the lying-in room, the "shew." Sometimes this flow amounts to two or three ounces, but this is exceptional, the rule being that it is just sufficient to thoroughly tinge the mucus with which it mingles. It therefore does not deserve the technical name of hemorrhage, and scarcely ever, we may even say never (unless injury has been done by the introduction of the hand or of instruments) will it do more than alarm a primiparous woman, and call for an assurance of the fact just stated, on the part of the physician.

(*b*) One of the symptoms of rupture of the uterus is a free escape of blood; but recall the terrible symptoms which mark that appalling accident, and you will see at a glance that they will at once remove the case from the classification of hemorrhage, and place it in that of the most fatal of the complications of labor. In other words, the gravity of the accompanying symptoms will mask this one entirely, and cast it completely into the shade.

(*c*) Rupture of the vessels, or of one vessel, of the funis umbilicalis, can at this day be no longer a matter of doubt, since in evidence of its occurrence appear the names of Delamotte, Levret, Baudelocque, Naeglé, Cazeaux, and many others. It is, however, a rare accident, fortunately for diagnosis, since there are no means other than mere absence of constitutional signs on the part of the woman, by which it could be differentiated from rupture of the utero-placental vessels.

(*d*) When the flow occurs from rupture of the vessels of the

vagina or bulbi vestibuli, it will generally have been the result of some violence, and our attention will likely be drawn to it by the sensation of pain on the part of the patient. Should it not, an examination, digatal or ocular, will readily reveal it.

The first of the four causes which have been so far examined into is sufficient to produce a flow really deserving of the denomination of hemorrhage; the second is accompanied by other grave symptoms which make this one a secondary matter; the third and fourth are of very rare occurrence, and it may be safely announced as a rule that *whenever, during labor, a hemorrhage occurs, it arises from partial separation of the placenta from the uterus, and consequent rupture of the utero-placental vessels.*

Varieties.—Generally the placenta is so placed in the uterus that the os may dilate and the child be expelled without its separation being involved in these processes, and it will, under such circumstances, retain its position and the integrity of its attachment, unless some untoword accident, such as a blow or fall, occur to displace it. At other times, however, it is attached to one side of the cervix, or over the entire cervix, so as to prevent the dilatation of this part, through which the child cannot pass as long as it remains closed. Now, as the os and cervix *must* be dilated to permit the passage of the child, and as their dilatation *must*, under these circumstances, to a greater or less extent, detach the placenta and rupture the utero-placental vessels, it follows, as a deduction, that hemorrhage thence resulting is not produced by accident, but, *ex necessitate rei*, is unavoidable.

For these reasons, all hemorrhages occurring during labor have been very properly divided into—

1st, Accidental hemorrhage;

2d, Unavoidable hemorrhage.

The second variety, you perceive, is synonymous with placenta prævia, an appellation which defines the unfortunate location of the afterbirth which produces it.

Leaving the subject of placenta prævia and its resulting unavoidable hemorrhage for our next lecture, I will occupy you today with the consideration of that variety which is purely the

effect of some accident, and which, like every other accident, might, under favorable circumstances, have been avoided.

ACCIDENTAL PARTURIENT HEMORRHAGE.

Frequency and Prognosis.—You will, I imagine, get a much more correct notion of the frequency of accidental hemorrhage, by an examination of the reports of one faithful observer, than by averaging a large number of cases collected in the loose and unreliable manner which ordinarily characterizes the accumulation of statistical evidence. Dr. Collins, during a mastership of the Dublin Lying-in Asylum of seven years, had 16,654 births occur under his supervision, and in this immense number only thirteen cases of this variety of flooding were met with; considerably less than one in one thousand. Small as this proportion is, however, it is larger than it should be for true accidental parturient hemorrhage, since Dr. Collins brought under the same head all those cases occurring during the three last months of pregnancy and during labor.

Of the thirteen women thus attacked, two died, and both after serious operations: one after version, and the other after craniotomy, so that it is by no means proper to conclude that they died from the hemorrhage. Of the children, one only was born alive.

Thus you will perceive that the accident is not of frequent occurrence, that the prognosis for the mother is good, and that that for the child is decidedly bad. I refrain from giving you other statistical statements on this point, from the fact that authors generally confound the two first varieties of hemorrhage together.

Causes.—The pathological state causing the flow, is, as already mentioned, rupture of the vessels which pass from the uterus into the placenta. The causes which bring about such rupture are numerous, since any kind of violence sufficiently great for the separation of the placenta would accomplish it.

The chief are blows or falls;

sudden uterine contraction from mental emotion;

sudden shocks or succussions given to the uterus, as from laughter, vomiting, &c.;

The chief are dragging off of the placenta by shortness of the cord, or its repeated winding around the child's neck.

placental apoplexy occurring near the periphery of the organ.

There are other and less frequent and conspicuous causes, but it would be useless to name them, since, as I have said, any accident which severs the utero-placental attachment would produce it.

Symptoms and Diagnosis.—As the prognosis, and more especially the treatment of the two varieties of parturient hemorrhage differ from each other very much indeed, it is of great importance that the accoucheur should determine at once as to which one he has to deal with, and that his decision be as far as possible positive and final. This he will, in many cases, do without difficulty, but sometimes he will have to remain in suspense for a short period, until the progress of the case enlightens him and determines the point.

Denman, on this point, justly remarks: "Before there is some dilatation of the os uteri, be the discharge ever so profuse—and it may even at this time be excessive,—I do not know that it is always possible to tell with certainty whether the placenta is present or not. It may indeed be conjectured that the placenta is there attached by the cushion-like feel of the cervix and lower parts of the uterus." He then goes on to remark how, even after dilatation of the os, a clot of blood may be mistaken for the placenta.

The only reliable means for determining the nature of the flow are these:

In Accidental Hemorrhage,

(*a*) There will have been no ante-partum loss,
(*b*) Uterine efforts will diminish the flow.
(*c*) An evident cause will generally be found for it.
(*d*) The loss is not generally very profuse.
(*e*) The placenta cannot be touched.
(*f*) Os uteri will be natural to the touch.
(*g*) Placental murmur loudest near fundus.

In Unavoidable Hemorrhage,

(*a*) There will have been hemorrhage during the last month or months of pregnancy.
(*b*) Uterine efforts will increase the flow.
(*c*) No cause will be found for it.
(*d*) The loss is often sudden and profuse.
(*e*) The edge of placenta may be touched.
(*f*) Os uteri will be thicker than ordinary.
(*g*) Placental murmur loudest in one or other iliac fossa.

As a little reflection will readily explain to you why these two varieties should be characterized by their respective symptoms, I will not do more than enunciate them. Let me insist, however, upon the importance of an early and positive diagnosis, if such is within the range of possibility. Of all the symptoms mentioned, the presence of the placenta near the os is the most valuable, and this one you must thoroughly test. Do not be satisfied with temporizing with digital examinations if they are not sufficient, but, explaining the necessity to your patient, pass the entire hand into the vagina; if the os is dilatable, pass the index finger well up into the cervical canal, and ascertain to your full satisfaction whether you have or have not a case of placenta prævia to deal with. As a matter of course, if the rational signs point strongly to the supposition that the case is one of accidental hemorrhage, and there is no immediate danger, you would not expose your patient to the annoyance and pain attendant upon this procedure; but far better would it be to err on that side, than by a culpable inactivity to remain ignorant of a point upon the knowledge of which so much will depend.

Treatment.—A parturient uterine hemorrhage should be treated upon precisely the same principles which should guide us in the management of such an accident taking place from any other part of the body. This you may, at first thought, regard as a sweeping assertion; but as we proceed, you will perceive that, although from the nature of the locality from which the flow occurs, the means employed for *developing* the principles may differ, the *principles* themselves are identical.

Let us suppose, for example, that a hemorrhage should occur from any part of the surface of the body, as the result of a wound or abrasion, and let us follow out the principles which one after another would be employed by the surgeon, until he finally succeeds in checking it.

1st. If the flow were slight the patient would be kept perfectly quiet, and an effort made to constringe the mouths of the bleeding vessels by cold and styptic applications, as ice, alum, tannin, matico, etc.

2d. Should these very useful and commonly employed hæmostatic agents fail in making this principle effective, an attempt might be made to cause in the wound the formation of a coagulum, which, extending up into the mouths of the bleeding vessels, might seal them up as is done by plugging the anterior nares alone, or with the posterior, in epistaxis.

3d. Should this fail, a very excellent principle, that of closing the open arterioles by firmly compressing their walls, might be developed by direct pressure, as is done, for instance, in hemorrhage from the palmar arch, by placing a billiard ball in the palm of the hand, and binding it firmly in its place by a bandage.

4th. Should even this fail, still another and surer one exists in the application of a ligature to the bleeding vessels; and to it the surgeon would now, with confidence, resort.

Thus, one after another he has brought to his assistance four principles, each valuable in itself, each differing from the one tried before it, and all capped by one which is as certain in its results as human means can ever be.

Thus, too, gentlemen, in parturient hemorrhage the obstetrician should act; and he will find that, if the first three of these four principles fail him, he, like the surgeon, will have one left which will prove as certain as the ligature.

In establishing these principles, always be mindful of the pathological state which causes the dangerous symptoms which they are to control; *i. e.*, that a portion of the placenta has been torn off from its uterine attachment, and that, from its disrupted face, as well as from the corresponding surface of the uterus from which it was torn, the blood is welling forth.

In a case of accidental parturient hemorrhage, the first indication to be fulfilled is to check the flow by constringing the mouths of these vessels; and this will best be accomplished by confining the patient to bed in the supine posture, and absolutely prohibiting all muscular effort or mental exercise, even that attendant upon speaking; by keeping the apartment cool; by administering cold, acidulated drinks, as lemonade, or water acidulated with the elixir of vitriol; by applying towels soaked with cold water, or vinegar and water, to the vulva and over the uterus, and by prescribing astringents, as tannic or gallic acid in full dose, which, being carried to the bleeding vessels by the circulation, may aid in producing the same result which their local application effects in vascular rupture elsewhere.

If by these means we succeed, we will have good cause for congratulation; for we will have relieved the woman without having in any way sacrificed the chances of her child. If they do not succeed, then we must resort to some other plan which may prove more effectual, and we enter into the consideration of the adoption of the second principle. The only available means at our command for causing a clot to form in utero, under these circumstances, is the tampon or vaginal plug, an agent advised by many, and one which might accomplish the result as perfectly as do the double tampons employed in epistaxis. But there are dangers attending its use so great, that I must not only guard you against them, but advise an avoidance of this means in parturient hemorrhage, except in rare and particular cases. I would say in advance, *avoid the tampon as a rule, after the seventh month of pregnancy, but employ it boldly, even at full term, in a few exceptional and peculiar cases.*

The tampon, gentlemen, may be styled one of the most useful and dangerous of our uterine hæmostatics, and it is really curious to see how different and even contradictory is the advice which is given concerning the propriety of its employment. Let me, by an excusable and called-for digression, endeavor to fix in your minds this morning a few maxims concerning it.

A plug introduced into the vagina, of sufficient size to fill the canal, acts in uterine hemorrhage in these two ways—preventing

the escape of the fluid which is flowing out of the uterus; this collects, and gradually is "backed" into the cavity above; soon it distends this cavity to its utmost extent: if the fœtal mass is present, insinuates itself between the chorion and uterine wall, and at last, forcibly dilating the os by distension of the whole organ, produces a powerful expulsive effort, which frequently expels child, accumulated blood and tampon together. When the uterus is not dilatable by the accumulating blood, this fluid coagulates within its cavity; the coagulum, beginning to form at the os, extends upwards towards the source of the hemorrhage, and will often seal up the mouths of the bleeding vessels.

Both these results are often very desirable, and to accomplish them, no means compares with the tampon. But after the seventh month of pregnancy the uterus is so large, that it may contain a sufficient amount of blood to produce death; so that from this period to the completion of labor, it is always attended by danger. (I need not insist upon the gross impropriety of the employment of such a means after delivery).

Thus, then, although the tampon might effect much for us in parturient hemorrhage, as a rule it should not be employed, and, in exceptional cases which demand it, should be resorted to only after mature consideration, and its effects be watched with very careful scrutiny. Observe these rules in using it.

Never employ the tampon *from choice* when there is a possibility of a dangerous internal hemorrhage.

At full term do not employ it after the waters have been discharged, for then the uterus will accommodate a large amount of blood.

Never employ it at full term after your patient has lost a great deal of blood, or from natural feebleness of body would be endangered by even a slight hemorrhage.

In a strong woman who has not already lost a good deal of blood, in whom the uterus is contracting well, and whose bag of waters has not been ruptured, I would not hesitate to employ it if other means failed, or from any reason I deemed them inapplicable.

Should the principle which is developed by the tampon be be-

yond our reach on account of the danger of the means which accomplishes it, or, should it have been unsuccessfully resorted to, how are we to avail ourselves of the third?

You remember that the flow of blood in accidental parturient hemorrhage is checked by uterine contraction, and that this is so marked as to constitute one of its characteristic symptoms: now let us examine this fact. When the organ contracts, the bleeding surfaces of the placenta and uterus are pressed firmly against the fœtal mass, and thus their open vessels are shut. If we could cause this pressure to be continuous and powerful, at the same time that it was resisted by a hard mass, we would cause the flow to cease entirely, and would be acting exactly as the surgeon does who binds the billiard ball in the palm of the hand. But you may ask how are we to introduce a hard resisting body into the uterus to act as counterpart of the ivory ball? We are supplied with such a substance in the body of the child. Surrounded by the soft and pliable bag of waters, one chief object of which is to prevent its hardness from being perceived by the sensitive uterus, it lacks the feature of resistance which we now desire; but evacuate the surrounding waters by puncturing the bag, and instantly the unyielding body presses against the hemorrhagic spot, and the principle is developed.

This, however, although often sufficient, is not always so, the pressure not being powerful enough. Under such circumstances, in the case of a palmar hemorrhage, the surgeon would remove his loose bandage, and apply another which would make all the pressure desirable. And so the obstetrician, by the administration of small doses of ergot, can so force his point of resistance against the bleeding surface as to compress entirely the ruptured vessels and render them impermeable. By these means you not only bring to your aid the principle mentioned, but, to a certain extent, you will establish that which will be mentioned fourth, for the vessels are diminished by the same contractions which press the child against the bleeding surface. According to my experience, it is rare for them to fail. In fact, I have never known them to do so in true accidental hemorrhage. Should they do so, however, but one resort remains, and that is ligation

of the vessels from which the obstinate current flows. Have we any means by which ligatures may be thus applied in utero? Again bountiful Nature comes to our aid, and we have but to use the means which she presents us, and our end is accomplished. After every natural labor, were there not some arrangement for checking the flow from the broken utero-placental vessels, a hemorrhage would occur; but so soon as the uterus is emptied, the fibres contract, diminish its size very greatly, and being arranged around the mouths of the vessels as the meshes of a nettled purse are around the finger which is pushed through them, they inevitably close these mouths, and prevent all sanguineous loss.

After having tried in vain, by the development of the three principles mentioned, to accomplish what we wish, naught remains but to empty the uterus, force it into contraction, and cause Nature to do what the surgeon does in external hemorrhage. If the head can be seized by the forceps, employ them; should it be out of their reach, accept of version as the alternative, and deliver promptly. Thus, by successive steps, the scientific obstetrician advances from mild, harmless, but correspondingly inefficient means, to more dangerous and proportionately more effectual ones, until he arrives at a point at which he can safely say, "I will by this surely succeed in staying the flow, and will rescue my patient from its dangers."

But do not despise the more inefficient means because a more effectual one exists. Would you not blame the surgeon, who, for a slight hemorrhage, should tie the supplying arteries without seeing what might be done by styptics, pressure, etc.?

Keep the most efficient agent in reserve, *because it is accompanied by danger for mother and child*, and always strive to accomplish your ends by the mildest, least dangerous, and apparently most trifling means. Should you succeed, a host of unthought-of evils, lurking like harpies in the shade, may by your moderation be avoided; should you not, then promptly apply the most efficient and most dangerous of your resources, which, like a "corps de reserve," you have kept until the fitting moment.

What has been denominated, gentlemen, "heroic practice,"

often marks the course of the ignorant and unreflecting obstetrician; and although the vulgar may be blinded by its show of energy, decision and promptness, and led to believe it an evidence of knowledge, it will often bring about consequences alike disastrous and avoidable.

The skillful general does not fire a twelve-pounder at a handful of marauders who could be dispersed by a musket-shot, nor does he trust to his muskets when an army is upon him in its might.

Never lose sight, too, of this fact in treating a complication of labor, that the interest of two beings are intrusted to your care, and that while you are to do all in your power for those of the mother, those of the child are scarcely less imperative. If, then, in the treatment of this accident, you can adopt means which will accomplish both ends, give them by all means the preference over those which, even if more surely effectual in removing the woman from danger, will sacrifice the chances of the child.

The older one grows in obstetric experience, the more convinced does he become that many a woman has died from the unnecessary introduction of the hand into the uterus; that many a uterus has been ruptured by uncalled for violence; and that Herod destroyed not a tithe of the children which have been killed in utero by the reckless use of ergot.

The following is a résumé of the treatment which has been recommended in this lecture, the principle upon which each procedure acts being italicised.

1st. *Constringe the bleeding vessels* by cold to the uterus and vulva, acidulated drinks, astringents, and perfect rest in the recumbent posture.

2d. In case of failure, *cause a clot to form in the mouths of the bleeding vessels* by the tampon, should the case be one in which this practice would be safe.

3d. Should this fail, *make direct pressure against the bleeding vessels* by evacuation of the waters, and increase it, if necessary, by the use of ergot.

4th. None of these means succeeding, *ligate the vessels* by evacuating the uterus, and causing firm contraction.

As I have alluded to certain cases in which the tampon might,

in a woman for whom we did not fear a slight loss of blood, be preferable to an immediate resort to rupture of the membranes, it may be well for me to give you an example. There are several cases where it might be preferable, but this will serve as a type: in a transverse presentation before the os is dilatable, rupture of the bag and administration of ergot would much complicate the operation of version, and thus endanger both mother and child. Should accidental hemorrhage occur in such a case, then it would be advisable to gain time for dilatation of the os by the use of a means which offers the probability of checking the flow without wasting the precious fluid which is to facilitate a dangerous operation.

Because this means is attended by danger, I would not entirely discard it; but let that be a sufficient reason for its not being employed, except when absolutely necessary, and for its effects being watched with the utmost caution.

American Medical Times.

TREATMENT OF RETAINED PLACENTA.

The following case, as illustrating Mr. Pemberton's evidence upon "Extensive Laceration of the Uterus and Vagina between the Fourth and Fifth Months of Pregnancy," especially the fourth point—namely, "that any such introduction of the hand into the uterus at the period of gestation (the fourth and fifth month) was uncalled for and improper," is at your service:—

Late in the night of May 17th, 1860, I received a message that D. C——, a farmer's wife, four months and a half advanced in her third pregnancy, was suffering pains and some amount of hemorrhage. Not considering it necessary to visit her from the nature of the message, I prescribed a mixture of infusion of roses, with additional sulphuric acid (diluted) and laudanum, enjoining strict rest in the recumbent posture, and visited her in the morning early, when I found the hemorrhage and pain had ceased. This state of things continued until Sunday afternoon, May 29th, when she got up and came down stairs. The result was a return of pains and flooding, which continued with more

or less severity until the following day. At thirty-five minutes past eleven A. M., I delivered her of a living fœtus, about nine inches long, whose heart continued to pulsate for nearly three hours after birth. I passed my finger by the side of the funis, and found the os uteri rigid, thick and unyielding, and was unable to touch the insertion of the cord without using more force than I thought desirable. I waited some time, and, finding neither pains nor hemorrhage, left her, with directions that I was to be summoned if flooding occurred. I watched her carefully; no untoward symptom presented; and on the 24th, the third day after the expulsion of the fœtus, the placenta was expelled during defecation, after the action of a dose of castor oil. This was attended by the expulsion of a few ounces of blood, and the patient made a most satisfactory recovery.

In this case I acted upon the advice of Dr. Blundell, than whom a more sagacious accoucheur never lived, who says: "If the placenta be retained, and the hand cannot be carried up so high as to command the placenta, *without the risk of bruising or lacerating*, leave it in the uterus; not because it is *not* an evil to leave it there, but because to leave it in the uterus is a smaller evil than to abstract it with violence; and we had better abide by the smaller evil than expose ourselves to the greater evil of lacerating, bruising or killing." See Blundell's Midwifery, edited by Dr. Severn, p. 407. *Cor. of London Lancet.*

VERSION WITHOUT INSERTING THE HAND IN THE UTERUS.

The two communications of Dr. Braxton Hicks, published in a former number of the Lancet, descriptive and illustrative of "a new method of version in abnormal labor," induce me to send you the details of the following case:—

On the 28th of January I was summoned to attend Mrs. ——, a farmer's wife, in her third confinement. On my arrival, I ascertained she had been in labor for several hours. The pains were strong, and occurring at regular intervals; the os I found dilated sufficiently to admit the forefinger; the membranes were entire. The presenting part I had little difficulty in making out

to be the left shoulder, in what I considered the first position. The slightest pressure on it with the finger caused the child to recede considerably, showing the liquor amnii to be abundant. Having now determined that the case could not with safety be left to Nature, I resolved upon having recourse to the operation of turning; but as the time for its performance had not yet arrived, I was content to remain some time longer passive, at the same time avoiding giving a direct answer to my patient's earnest inquiry if all were right, for I knew I should encounter many difficulties were she apprized of the abnormal position of the child. On again making an examination, I found matters as I had previously left them, with the exception of the os, which had expanded to the size of a half-crown piece, and would, if required, admit with little difficulty the whole hand.

From the very great facility with which the fœtus could be raised and moved about, it occurred to me that its position might be improved without either introducing the whole hand or rupturing the membranes—in short, that it might be possible, without having recourse to either expedient, to bring down the head into the brim of the pelvis, so as to convert the case into a natural presentation. With this object in view, I endeavored to raise up the presenting part with two of the fingers of the left hand, while with the right I made steady pressure upon the head in the direction of from left to right. In a very short time the shoulder began to recede, but only to be replaced by another part of the child. The head could now be felt approaching slowly the mesial line. The external pressure was continued, while the fingers of the left hand were employed in guiding the head towards the pelvic brim, into which, before I had withdrawn the hand, it had entered. The case, being now converted into a natural presentation, was left to Nature. In course of time a fully developed and healthy male child was born. The whole operation was completed in less than five minutes, and without the knowledge of the mother—a matter of no small moment in the present instance. No doubt this mode of version was greatly favored by the peculiar circumstances of the case; and, although

it may be applicable in only a very few, it seems to me by far the safest, both to mother and child.

I send you the above observations, which show, as I think, that some cases of malposition of the head may be remedied according to the "new method," without causing the child to perform a complete evolution, in the hope that others may be induced to record their experience on the subject, so that a proper conclusion may be arrived at regarding the merits or demerits of the "new method of version." *Cor. of London Lancet.*

I think the following case so fully confirms the value of the method of turning described by Dr. R. Hicks, that I beg its insertion in your journal:

Early on the morning of July 24th, I was summoned by a midwife in this neighborhood to go over to Woking to see a young woman under her care, and who was in a dangerous state of flooding. I found the patient, a delicate person, aged twenty-eight, in labor with her fourth child, in a most critical condition; face and lips pallid; extremities cold; pulse 140, small and tremulous. She had only three slight pains, but had lost a large quantity of blood. I found the vagina filled with coagula; the os uteri dilated to the size of a crown-piece, and entirely blocked up by the placenta. On passing two fingers between the wall of the uterus and the placenta, I felt a cephalic presentation, the membranes being unruptured. Knowing that no time must be lost, I determined on trying the plan of version suggested by Dr. B. Hicks. With my two fingers I made steady pressure on the head, while, with my left hand placed on the fundus uteri, I bore down the nates by gentle impulsive pressure. The head at once receded, and in two or three minutes I felt a foot presenting. Rupturing the membranes, I seized it, and directly afterwards the other; and, with a little firm traction, brought the breech down into the cervix. I now waited, and gave brandy and gruel. My patient said she had scarcely felt what I had done to her, and appeared more comfortable. In ten minutes there was a slight pain, when I delivered her of the child (still-born); the uterus contracted freely, and in five min-

utes I removed the placenta, no hemorrhage following. The patient remained in a rather alarming condition for some hours, but, by the use of stimulants, &c., rallied, and has since entirely recovered.

I believe that the patient was saved much distress and pain, and that the shock to the system was greatly diminished, by my not being obliged to pass my hand entirely into the uterus; and further, had I done so, I must have separated a larger portion of the attached placenta, whereby there would have been more hemorrhage, which she could not have borne. To me it has been a most satisfactory case, and I think it bears strong testimony to the value of the method.—*Ibid.*

ON THE DIATHETIC DISEASES OF CHILDHOOD.

By William Jennee, M.D., Physician to University College Hospital, and to the Hospital for Sick Children.

[The great diathetic diseases of childhood are four, viz., rickets, tuberculosis, scrofula, and syphilis. All are liable to produce striking and peculiar deviations from the healthy structure of particular organs and tissues. The following is a brief enumeration of the leading features of a typical case of each of these four conditions:]

Tuberculosis.—Nervous system highly developed; mind and body active; figure slim; adipose tissue small in quantity; organization generally delicate; skin thin; complexion clear; superficial veins distinct; blush ready; eyes bright, pupils large, eyelashes long; hair silken; face oval, good-looking; ends of long bones small, shafts thin and rigid; limbs straight. Children the subjects of tuberculosis usually cut their teeth, run alone, and talk early.

Leading Pathological Tendencies.—Fatty degeneration of liver and kidneys; deposits of formation of tubercle, and their consequences; inflammation of the serous membranes.

Scrofulosis.—Temperament phlegmatic; mind and body lethargic; figure heavy; skin thick and opaque; complexion dull, pasty-looking; upper lip and alæ of nose thick; nostrils ex-

panded; face plain; lymphatic glands perceptible to touch; abdomen full; ends of the long bones rather large; shafts thick.

Leading Pathological Tendencies.—Inflammation of the mucous membranes of a peculiar kind; so-called strumous ophthalmia; inflammation of the tarsi: catarrhal inflammation of the mucous membrane of the nose, pharynx, bronchi, stomach and intestines; inflammation and suppuration of the lymphatic glands on trifling irritation; obstinate diseases of the skin; caries of bone.

Rickets.—Mental capacity and powers small; muscular force deficient; mind and body inactive; figure short; closure of the fontanelles retarded; face small, but broad; skin opaque: often set with downy hairs. Children the subjects of rickets are late in cutting their teeth, in running alone, and in talking, and their teeth drop early from their sockets.

Leading Pathological Tendencies.—Softening of the bones; enlargement of the ends of the long bones; thickening of the flat bones, and deformities consequent on these conditions of the bones; so-called hypertrophy of the white matter of the brain; chronic hydrocephalus; pulmonary collapse; laryngismus stridulus; convulsions: albuminoid infiltration of the liver, spleen, lymphatic glands, &c.

Syphilis.—Adipose tissue small in quantity; muscles flabby; cutis rough—deficient in contractility; complexion muddy.

Leading Pathological Tendencies..—Suppurative inflammation of the mucous membrane of the nose; ulceration of the mucous membranes of the nose and of the lips, mouth, throat and anus; falling of the hair; eruptions on the skin of a peculiar character; induration of the liver; suppuration of the thymus, lungs, &c.

There are pathologists of high repute who regard rickets, scrofulosis and tuberculosis to be mere modifications of the same disease. I and others hold them to be distinct affections. The whole difficulty of the question lies in the difficulty of determining what is necessary to constitute identical diseases. With reference to some diseases the grounds of separation are broad and unequivocal. Scarlet fever and measles are distinct diseases

because their specific cause is different. Syphilis and tuberculosis are undoubtedly not identical, because syphilis owes its origin to a specific cause, and tuberculosis does not. Rickets, tuberculosis and scrofulosis are due, it is said, to mal-nutrition, and therefore it is urged they are essentially identical; but it is manifest that the term mal-nutrition is at once very vague and very comprehensive. All diseased action by which unhealthy structure is formed in the place of healthy, may be called mal-nutrition. Pus is the result of mal-nutrition, cancer is the result of mal-nutrition, tubercle is the result of mal-nutrition,—and yet the purulent, cancerous, and tuberculous diathesis cannot be held to be identical.

I hold rickets, tuberculosis and scrofulosis to be distinct diseases in the sense in which tuberculosis and cancer are distinct diseases, and for the following reasons:—

1. Because the general condition in each is perfectly different from that in the other.

2. Because the pathological tendencies of those who manifest these different general conditions are different.

3. Because we rarely see the pathological tendencies of the one manifested by those, the subject of the others, *e. g.*, rickets is absolutely unfavorable to tuberculization—tuberculization to strumous ophthalmia.

4. Because, tuberculosis being unquestionably hereditary, we do not find the children of phthisical parents especially prone to rickets or to scrofulosis.

5. Because, although we often find several members of the same family the subjects of rickets, of tuberculosis, or of scrofulosis, it is comparatively rare for members of the same family to be the subjects of more than one of these diatheses.

6. Because rickets is not hereditary in the sense in which tuberculosis is hereditary.

7. Because the course, prognosis and treatment of each of these diatheses are different.

Med. Times and Gazette, March 17, 1860, p. 259.

ON RICKETS.

BY WILLIAM JENNER, M.D., ETC.

[Though generally met with among the children of the poor, in large towns, rickets is occasionally seen in those very circumstances least likely to produce it, viz., in the children of the wealthy, living in good country air.]

Rickets is a general, or diathetic disease, manifested after it has existed a longer or shorter time, by certain lesions of the structure of the bones,—and, I say, manifested after a time, because in some cases the general disorder unequivocally precedes the local changes, and before these latter occur, we can predict that they will occur.

In some books, rickets is classed among diseases of the bones. This is a mistake; rickets is no more a disease of the bones, than is typhoid fever a disease of the intestines. Rickets leads to disease of the bones in the same way that typhoid fever leads to disease of Peyer's patches; but there is a general disease preceding and accompanying the disease of the bones in the one case, as there is preceding and accompanying the disease of the intestines in the other. The change in the bones is the anatomical character of rickets.

The most constant and striking anatomical lesions in rickets are—

1. Enlargement of the ends of the long bones, of the parts where the bone and cartilage are in contact, *i. e.*, where the cartilage is preparing for ossification, and where ossification is advancing in the cartilage.

2. Softening of all the bones.

3. Thickening of the flat bones, *e. g.*, the bones of the skull —the scapula.

4. Deformities which follow from mechanical causes acting on the softened bones, *e. g.*, the deformities of the thorax, pelvis, spine, long bones.

5. Arrest of growth, not only of the bones, but of all the parts directly related anatomically and physiologically to the bones, *i. e.*, of the muscles, vessels, nerves and teeth.

6. Certain lesions of the pericardium, lungs, and capsule of the spleen, the direct consequences of the thoracic deformity.

7. Less constant, but highly important changes, most commonly affecting the nutrition of the brain, spleen, liver, lymphatic glands and muscles, and now and then of every organ.

[Dr. Jenner then passes on to examine at greater length the anatomical lesions of the bones, the consequences of this disease.]

1. *Enlargement of the Growing Ends of the Long Bones.*—[Dr. Jenner, under this head, by actual measurements in several cases, shows that the enlargement of the ends of the bones is actual, and not apparent only, as asserted by some.]

As to the intimate anatomical structure of the large ends of the bones: there is excessive formation of the structures which precede or form the nidus for ossification; there is retardation and incomplete performance of the process of ossification. In the healthy child the ends of the long bones measure more in circumference than the shafts; as the process of ossification is completed, the bone diminishes in girth.

In rickets there is an exaggeration of the condition we find in the first stages of ossification in the healthy subject; the completion of the process only is stayed. There is great development of the spongy tissue of the head of the bone, and of the epiphyses, and also of that layer of cartilage in which the primary deposit of calcareous matter takes place.

The layer of cartilage in which the cells are arranged in linear series, instead of being half a line, is from a quarter to half an inch in breadth.

Again, the calcareous granular deposit is wanting at the boundary of ossification, and there the cartilage-cells calcify before the matrix; the consequence of this is that you see the cartilage-cells, being apparently converted into lacunæ and imperfect canaliculi.

You may easily trace all stages in the deposit of calcareous matter on the inside of the cells, from that in which it forms a mere ring to that in which it is so thick as to leave only a vacant space resembling an almost perfect lacuna. Kölliker has generalized from his observations on ricketty bone, thinking that in rickets the normal process by which the lacunæ are formed is visible. My observations lead me to quite another

conclusion: the calcification of the cartilage-cells in the growing cartilage in rickets seems to me identical with the calcification of the same parts occasionally seen in enchondromata. It is a pathological process—a petrifaction. The spongy tissue is much more spongy in appearance than natural, and from the interstices of its meshes a deep red pulp is expressible. This pulp is composed of colorless nucleated cells usually containing only one nucleus, now and then two, and occasionally several blood-globules, and in some cases a very large quantity of free fluid fat. If, as Sharpy, Somes and De Morgan have supposed, these cells play an important part in the completion of the process of ossification—and I am, from my own observation on rickety bones, &c., inclined to think they do,—we see in their abundance in rickety bones only a further evidence that in rickety bone there is excessive preparation for the process of ossification and arrest of the completion of the process. The periosteum is thickened over the head of the bone as over the bone generally. It attains its maximum degree of thickening just at the point of junction of the bone with the cartilage.

The periosteum of the whole bone is often more vascular and thicker than natural, but in the many post-mortem examinations of extreme rickets which I have made, I have never seen any bloody fluid, as described by Guerin, beneath that membrane. A crimson pulp fills the canal, and all the interstices of the tissues of the long bones. It is composed of elements identical in appearance with those expressible from the spongy tissue of the head of the bones. In this pulp, however, I have never observed any such quantity of free fluid fat, as I have noted in the heads of the bones near the line of progressing ossification.

2. The softening of the bones is sometimes so great, that the bones which in their healthy condition are the strongest, may be bent by the most trifling force; and those which naturally are the thickest, may be cut by a knife with facility.

This softening of the bones is chiefly the consequence of the diminution of their earthy salts. It seems, however, from the experiments of Lehmann and Marchand, that the animal mat-

ter of rickety bones differs, in some cases, from that of healthy bone; for, in some of their experiments, the bones yielded no gelatin on boiling.

Putting together the results of the analyses of several observers, we gather that the bones of healthy children yield about 37 parts of organic and 63 of inorganic matters; and that those of rickety children yield about 79 parts of organic and 21 of inorganic matters.

3. The thickening of the flat bones is the consequence of thickening and increased vascularity of the periosteum, and of the abundance of the nucleated cells, which, with blood from the pulp, occupies all the meshes of the bone. The thickening is usually greatest just within or at the growing margin of the bone, so that in the cranial bones it is greatest near to or at the sutures, and least at the centres of ossification.

4. *Deformities which follow from Pressure on the Softened Bones.*—The spine is bent; the cervical anterior curve is increased; the face is directed upwards; and the head falls backwards. This curvature is only strongly marked when the muscular debility of rickets (of which I shall speak hereafter) is very decided. The vertebræ being softened and the muscles weakened, the head is no longer supported, and it falls forwards or backwards as circumstances may determine. Usually the child favors the falling of the head backwards, in order that it may see what is going on around, just as when, suffering from paralysis of the third nerves, the child sits or walks with the head thrown backwards, in order that it may see under the fallen upper eye-lids. There is a posterior curvature of the spine, if the child is unable to walk, commencing at the first dorsal and extending to the last lumbar vertebra. If the child walks, then the posterior curvature is limited to the dorsal region, and there is an anterior curvature in the lumbar region. This posterior curvature in the child yet in arms, is sometimes so extreme that it may be easily mistaken (and I have known it mistaken) for angular curvature. They are distinguished thus:—

If the child be held by the upper part of its trunk, the weight of the lower limbs will usually remove the rickety

curve, and it may certainly be straightened if the nurse hold the child by the upper part of the trunk and the physician raises the lower limb with one hand, and at the same time places the other on the curved spine.

This curvature of the dorsal and lumbar spine in rickets is the consequence of the muscular weakness and softening of the bodies of the vertebra; its direction is determined by the weight of the head, etc. It is an exaggeration merely of the curvature always existing when the child of three or four months old is sitting unsupported on the nurse's arm. Lateral curvatures in the young child are less common than the antero-posterior. Their direction is determined by the posisition accidentally assumed by the child. For example, if the child be carried on the left arm constantly, there is a disposition to lateral curvature, and the convexity of the curve will be towards the left.

The femur is curved forwards and outwards. This curvature is produced before the child walks, by the weight of the legs and feet. The child sits on its mother's lap, or on a chair, and the lower extremities hang pendant. The bone being flexible, yields. After the child walks, the weight of the trunk is the chief agent in determining the curvature of the femur. The curve which existed before walking will be exaggerated.

When the tibia curves before the child walks or has been placed on its feet, the curvature is almost always outwards—an exaggeration only of the normal curve in the young child—and is produced by the child sitting somewhat cross-legged, and bearing on the floor or bed with the outer malleolus. After walking, the weight of the trunk is the chief agent in determining the bending of the tibia, and the direction of the curve will depend on the circumstances which determine the point on which the chief amount of pressure is brought to bear.

The curvature of the ulna and radius has been attributed to muscular action—to the child placing its arm around the breast of the mother, etc.; these causes are altogether inefficient for its production. The curvature of the bones of the forearm is produced by the child, owing to want of muscular power to support itself in the sitting posture, throwing part of its weight on to its

arms. The child places its open hands on the chair, bed or floor, and throws a large share of the weight of its trunk on to the bones of the upper extremities; the bones of the forearm are twisted as well as curved outwards.

The humerus is sometimes bent at an angle just where the deltoid is inserted. This curve is produced by the weight of the arm when the limb is raised by the action of the deltoid, and is increased by the cause which determines curvature of the forearm.

The clavicles are often the subject of extreme angular curvature. The chief bend is always at the same spot, viz., just outside the part to which the sterno-cleido-mastoideus and the pectoral muscles are attached. The second bend is about half an inch from its scapular articulation. The first curve is forwards, and somewhat upwards; the second backwards. The curvature of the clavicle is produced partly by the weight of the arm on the humeral end of the clavicle—the sternal end being supported by the muscles just mentioned, and by its ligaments,—but chiefly by the force brought to bear on it, when the weight of the trunk is thrown on to the upper extremities, the child being in the sitting posture, with the hands on the floor or crawling.

The deformity of the greatest interest to the physician is that of the thorax. The back is flattened. The ribs are bent at an acute angle where the dorsal and lateral regions unite. At that part the lateral diameter of the thorax is the greatest. From it the ribs pass forwards and inwards to the point where they unite with their cartilages; on that line the lateral diameter of the thorax is the least, the cartilages curving outwards before turning in to unite themselves to the sternum. The sternum is thrown forwards, and the antero-posterior diameter of the thorax is abnormally great. The consequence of the direction of the ribs being inwards, and of the cartilages outwards, is, that the thorax is grooved from above downwards on its antero-lateral from the 1st to the 9th or 10th rib; the deepest part of the furrow being just outside the nodes formed where the ribs and cartilages unite. This groove extends lower on the left than on the right side, but it is deeper on the 5th and 6th ribs on the right

than on the left side; the heart and the liver respectively supporting, to some extent, their corresponding ribs. The points of maximum recession correspond to the 5th, 6th and 7th ribs. A little below the level of the nipple the chest expands considerably, the chest-walls being borne outwards by the liver, stomach and spleen. If we examine the thoracic walls from the inside, the appearance is most remarkable; where the ribs join with the cartilages, there are much greater projections than on the outside; but the 11th and 12th ribs, which are not inflexed, have the same enlargement on the inside as on the outside.

The great determining cause of the thoracic deformity is atmospheric pressure; this is aided by the elasticity of the lungs. How is the pressure of the atmosphere brought to bear on the thoracic parietes? Suppose the thoracic external parietes were made of cast-iron or any other unyielding material, then the diaphragm could descend only so fast as the air could enter at the orifice of the larynx, and overcome the elasticity of the lungs. The thoracic parietes, however, in their normal condition, are not absolutely unyielding; but then there is a due relation between their strength, the power of the diaphragm and the rapidity of its contractions, the size of the orifice of the larynx, and the elasticity of the lungs. The chest-walls being healthy, and the orifice of the larynx of normal size, if the young child sobs violently—*i. e.*, contracts the diaphragm with abnormal rapidity and force—the most flexible parts of the thoracic parietes will fall in during inspiration.

If the orifice of the larynx be narrowed, and if the diaphragm contract with only normal rapidity and force, there will be recession of the softer parts of the chest-walls at each inspiration.

Again, if the orifice of the larynx remain normal, the diaphragm act as energetically as in a healthy child, and the chest-walls be softened, then, at each inspiration, there will be recession of the most yielding part of the thoracic walls.

It is this last condition which exists in the rickety child. The part of the rib where the ossification is imperfect and incomplete is so soft, that at each descent of the diaphragm it recedes, and the furrow of which I have spoken is the consequence. Just

in proportion as the ends of the ribs are forced inwards the sternum is carried forward, and the consequence is the remarkable form of thorax of which a model is on the table, of which some examples are now in the Hospital, and of which it is rare to pass a day without seeing cases in the out-patients' room. Rokitansky has maintained that this deformity of the thorax is the consequence of the want of power in the inspiratory muscles. I have repeatedly dissected subjects in which this deformity was strongly marked, and find that there is no correspondence between the points of insertion of the muscles of inspiration attached to the outer surface of the chest-walls and the points of recession. The preparations on the table bear out my assertion. Weakening of these muscles would have as its consequence general want of expansion of the lungs, and the thorax would have the form that it obtains in senile atrophy—viz., long, with narrow antero-posterior and lateral diameters.

Again, the diaphragm is said to cause the circular recession by its direct action—by drawing in the receding parts at each contraction. On the table are some dissections, which prove, when compared with the cases and the model, that the line of recession does not correspond to the points of attachment to the diaphragm. But it does correspond to the upper margin of the liver, spleen and stomach, and is produced—as the longitudinal furrow is—by atmospheric pressure, the parts of the parietes below being prevented receding by the organs I have just mentioned. The influence of the organs beneath, in preventing the recession of the chest-walls, is illustrated by the *apparent* bulging of the precordial region in every case of well-marked rickety thorax. The chest-walls covering the heart do not recede so much as on the opposite side, and the consequence is that the left side is much fuller than the right; and, at first sight, it might be supposed that there was abnormal fullness of the precordial region.

In excluding muscular action from all direct share in the production of curvature of the long bones in rickets, I am, so far as I know, unsupported by any authority.

Med. Times and Gazette, March 17, 1860, p. 261.

DIAGNOSIS OF CARCINOMA UTERI.

BY DR. J. Y. SIMPSON.

As to the differential *diagnosis* of carcinoma uteri, I have little to say regarding it at present; for you have not yet had an opportunity of being made acquainted with the other diseases with which it is liable to be confounded. But there is one observation which I should wish to urge upon this point; it is this: that you are not entitled to diagnose the existence of cancer in the womb from any degree or kind of pain, bleeding, and offensive discharge, from which the patient may be suffering, not even when these are combined with the pale cachectic look which is usually pathognomonic of the disease. All these local and constitutional symptoms may be present in other uterine diseases besides cancer; and, in some latent cases of cancer, they may be found absent. You can detect it with perfect certainty only by physical diagnosis. You must take a vaginal examination with the finger—for the speculum is here of little service,—and only when the sense of touch has assured you of the condition of the cervix uteri, and not till then, can you honestly and conscientiously pronounce upon the nature of the case.

One morbid condition of the cervix uteri with which cancer of the organ may be, and often is confounded, is that which results from chronic inflammation in it. This causes enlargement of the cervix and expansion of the os, attended with great induration; and when ulceration sets in, and pain and menorrhagia are developed, the case may very readily be mistaken for a case of cancer in its first stage. But here you have always the distinguishing characteristic to which I have already alluded, that in chronic inflammatory induration of the cervix the deposit is confined to the organ itself, which remains loose and mobile; whereas in the case of cancer the surrounding tissues usually become early involved in the disease, and being infiltrated with the morbid deposit, make the cervix uteri feel firm and fixed. And when ulceration has set in, you will find that the ulcer which results from inflammatory change—though it may look very irregular, rough and ugly—is always on a level with the unbroken surface,

or even projects above it, whereas the cancerous ulcer is always depressed, excavating, as it were, and eating out the part in which it has its seat. One often sees mistakes made with regard to the diagnosis of the cancer of the uterus, which are almost unconceivable, and which you may, in almost every case, avoid if you will just keep your wits about you when making an examination, and use a little common sense—a most invaluable and indispensable aid in all kinds of diagnosis. When you feel a rough, irregular, excavated or anfractuous ulcer seated on a hardened base, and surrounded by hardened tissue in any other part of the body, as the exterior of the chest, face, or extremities, you set it down at once as cancer; and when the finger applied to the os uteri recognizes the same condition, you need not doubt that here, too, you have to deal with the same disease. Cancer of the uterus used often formerly to be confounded with simple polypus uteri; and Dupuytren and other Parisian surgeons have recorded instances of patients being sent to them from the country, who had been told by their own doctors that they must inevitably die of their complaint, and who were completely cured by the simple operation of the removal of the polypus, and were thus recovered, as it were, from the very brink of the grave. I have seen the same mistake frequently committed in our own times, and yet it is a mistake which care will always enable you to avoid. *Med. Times and Gazette, Jan.* 15, 1859, p. 54.

Premature Labor, *Induction of.*—(By Prof. Braun).—The following is the mode of procedure by Prof. Braun, of Vienna: Soften in hot water the end of a well-oiled catgut bougie, a foot long, and from two to three lines thick, and pass it along the index finger with a twisting movement into the uterine cavity, until only a portion, equal to two fingers' breadth, remains in the vagina. The bougie so passed always excites pains in from six to twenty hours, does no injury to the membranes, and is to be removed only just before the discharge of the waters, or the birth of the child.

Bibliographical Notices.

An Elementary Treatise on Human Anatomy. *By* Joseph Leidy, M.D., *Professor of Anatomy in the University of Pennsylvania.* Philadelphia: J. B. Lippincott & Co. 1861.

We are indebted to the publishers for a copy of the above work, and we must say that, as an Elementary Treatise, it is superior to anything we have yet seen on the subject—indeed, it is, in every respect, all the student could desire. The publishers certainly deserve all praise for the execution of this volume. The typography is most beautiful, the illustrations really exquisite, and the paper of the very best quality.

The Physician's Pocket Memorandum for 1861. *By* C. H. Cleaveland, M.D., *of Cincinnati.* B. Frankland.

This competitor for public favor possesses many merits to commend it to the profession. It is larger than any of the other publications of the kind, and is very well gotten up.

We take pleasure in calling the attention of our readers to the following

CIRCULAR.

The undersigned proposes to issue a yearly volume, with the following title:—

"YEAR BOOK OF AMERICAN CONTRIBUTIONS TO MEDICAL SCIENCE AND LITERATURE."

It is designed that Part First, of each volume, shall comprise an arranged and classified Summary of, and index to, all the important original papers found in the various Medical Journals of this country, for the year immediately preceding. Part Second will comprise a Summary of, and index to, all papers found in the published transactions of the National and the various State Medical Societies. Part Third will embrace Reviews of all medical books of American authorship, published during the year, with a Summary of all the novelties in opinion or practice therein. A copious index will complete the work. To the above plan and arrangement, such other additions shall be made as time and circumstances may suggest. The first volume will be issued early in the year 1861.

For the preparation of our Monthly Summary of American Journalism, for the "A. M. Monthly," we have arranged for all the American Medical

Journals [over thirty-six in number] and at least four of the best European. To facilitate our design, we request a *continuance* of the *exchange.*

American authors of medical works, and publishers of the same, are requested to send to our address a copy of their respective works and publications. Medical Societies that publish their Transactions will, we trust, be kind enough to send a copy of the same to our address. For this kindness of an exchange from Journal publishers, authors, book publishers, and Presidents of Societies, we shall be happy to reciprocate by an *exchange of publications.*

The importance of a work of the character as above, which shall comprise *all there is of interest* in the more than thirty Journals, medical books issued for the year, and the various society transactions, will be readily conceded by all. We cannot prepare the work and publish it at a pecuniary loss, and hence the object of this Circular is to request that all physicians who would encourage the work, and become subscribers to the same, would send us their names *at once*—payment to be made only on the publication of the work. The work shall contain from 500 to 800 pages, to be substantially bound, and furnished at the very low price of three dollars. That we may know whether the work is to receive sufficient encouragement to justify its completion and publication, we request that subscribers' names *may be sent in immediately.* As a special favor, we request that our friends will not allow a day to pass before responding to this Circular.

If Journal publishers will take sufficient interest in this National Enterprise to publish our Circular, we shall be happy to remunerate, should the enterprise more than pay expenses.

All Books, Journals, Published Transactions and Names of Subscribers, should be directed to

O. C. GIBBS, M. D.
Frewsburg, Chautauqua County, New York.

Errata in the Essay on the "Use of Tobacco," etc., by J. *F.* Shaffner, in the last number:—

Page 429, line 10 from top, for "*alenses,*" read, *olentes.*
" 430, " 9 " for "packet," read, *pocket.*
" 431, " 2 " for "hallowed," read, *hollowed.*
" 431, " 9 from bottom, for "20,000," read, 120,000.
" 431, " 8 " for "30 or 40,000," read, 30 *or* 40,000,000.
" 433, " 12 from top, for "recks," read, *reeks.*
" 436, " 3 " for "tumors," read, *tremors.*
" 438, " 5 from bottom, for "their," read, *thin.*
" 440, " 10 " for "$300,000," read, $3,000,000.
" 442, " 17 from top, for "awakens," read, *weakens.*
" 448, " 8 " for "immediate," read, *immoderate.*
" 449, " 8 from bottom, for "fire," read, *wire.*

Editorials and Miscellaneous.

VALEDICTORY.

My connexion with the Medical Journal of North Carolina terminates with this number. For two years of anxiety and toil, my energies have been devoted to its interests; and I now surrender it to abler hands, rejoicing in the reflection that, in despite of all opposing obstacles, the enterprise has proved a most decided success. Of the trials encountered, the difficulties overcome, and the apprehensions experienced in the establishment of the Journal, this is not the occasion to speak; and yet I must be permitted to say, that to one unsustained by partial friends and a most hopeful spirit, the task would have been too onerous to admit of aught else than a thorough and disastrous failure.

To the kind friends from whom I have received so many evidences of affectionate regard,—to those whose words of commendation and encouragement have sustained me in the darkest hours of my editorial life,—to the patient, faithful and forbearing patrons of this Journal, I desire to make a few remarks before saying "farewell" forever.

I am about to remove to another State, but, be assured, that I shall carry with me a most grateful and abiding recollection of the kindness I have received at your hands; and that whatever fortune may be mine, my lips shall never frame a more fervent prayer than that which they utter for the personal and professional prosperity of each one of you. If I have given offense at any time in the discharge of my editorial duties, either by omission or commission, believe me, when I protest, that there has been no malice in my heart, and accept this assurance as a full atonement for everything that has borne the semblance of an unkindness or indiscretion. And should business or pleasure ever carry you to my new home, I beg you, as a personal favor, to remember that there is one door in Baltimore which stands ready open to receive you, and a grateful heart within to give you a cordial, honest, Carolina welcome.

I shall not pretend to urge you to greater energy and diligence in behalf of that profession to whose service your whole lives have

been consecrated, since the history of the State, for the last few years, conclusively shows that Medicine has had no more devoted champions than the physicians of North Carolina. And as regards the interests of this Journal, I can only ask you to extend to my successors the open-handed generosity which has characterized your treatment of me, with the full assurance that there will be thus secured to it a still wider field of usefulness and a more complete success.

Whatever may have been the defects of the work I have had in hand, a want of loyalty to my native State has not been one of them; for I claim to be second to no man in love and devotion to North Carolina. Upon her soil I was born, and in her bosom I hope to sleep when my earthly task is done. To me her skies are the brightest, her forests the grandest, her rivers the most beautiful, her fields the greenest, her sons the noblest, and her daughters the most lovely, of all the world beside. There is not a flower that blooms amid her broad savannas,—there is not a stone upon her rugged mountains,—there is not a foot of her soil, from Buncombe to the sea,—there is not one of her children, be he humble or exalted, but has a place in my heart. And in all that concerns her honor or her interest,—in everything that tends to exalt and advance her,—I have felt from my earliest youth, and shall feel to my latest breath, a degree of solicitude which words cannot express or even deeds portray. With such sentiments in my heart, I feel assured that the pages of the Journal must have reflected something of them, and that no man can assert to the contrary. Indeed, it has been my delight to make the work essentially Carolinian, the representative of the talent of the State, the exponent of the sentiments of her sons, and the unwavering advocate of all that was calculated to promote her interests and to vindicate her character. Surely, then, you will pardon the egotism of the assertion that, in this regard at least, I have done my whole duty, and that the Journal has been loyal to the interests of North Carolina.

It only remains for me to say "adieu," and to wish you, one and all, every blessing that Heaven can bestow.

EDWARD WARREN

A REQUEST.

Will those who have paid up for the year be so kind as to drop a line to Dr. W. A. B. Norcom, Edenton, N. C., who will immediately forward, in return, the necessary receipt? There has been so much confusion in the affairs of the Journal for the last few months, that it is impossible for us to publish a list of payments.

The University of Maryland commenced its session on the the 15th of the last month, with an Introductory Lecture by the Professor of Materia Medica and Therapeutics.

The Class is one of the largest that ever assembled in Baltimore, and everything promises a most harmonious and successful session. We are pleased to state that North Carolina is well represented, and that these students are from most of the southern States. To our personal friends everywhere, who have interested themselves in our behalf, we return our most cordial thanks, assuring them that their kindness is fully appreciated; and that, should an opportunity ever offer, it shall be returned with interest.

Prurigo of the Vulva.—(By Prof. Simpson).—This may be relieved, and generally cured, by the assiduous and persevering application of a solution of borax, five or ten grains to the ounce of water, or in a little infusion of tobacco. An ointment of the iodide of lead, or of bismuth and morphia, is very useful. Chloroform applied locally, in the form of vapor, is one of the most certain means which can be employed. These remedies should be alternated. In the more obstinate cases astringents will be found of much use: alum, or aluminated iron, or tannin. Of course the general health of the patient must be attended to. Arsenic, aqua potassa, and other alterative medicines of that class, may be required.

DR. HOLMES *VS.* THE MEDICAL PROFESSION.

The Anniversary Meeting of the Massachusetts Medical Society produces a great stir among the fraternity of the Bay State, and many a hard-worked poor fellow whiles away his long and chilly ride with pleasant expectations of the good time coming.

Never was this anticipation more generally felt than on the approach of the last festival, held in Boston during the sweet sunny days of May. Nothing like it before. The councillors met, and the censors conferred together. The anniversary chairman announced his programme, and all the profession was alive with excitement. They should hear an address from that funny Dr. Oliver Wendell Holmes. What a treat! You'll laugh yourself to death! He is really afraid to be as funny as he can! And then, such a jolly dinner in the cradle of liberty!

Now, it is a cardinal principle, in all born of Massachusetts, to think that Boston is *the* city, and Dr. Holmes the smartest little man therein. Said Doctor has told them so, and they all agree with him. Isn't it the hub of the solar system? and though good Americans generally, when they die, go to Paris, every true-blooded Yankee would be satisfied to spend his here and hereafter in Boston.

So, in they came from all directions, with their wives and rosy daughters, with their wonderful cases and tedious statistics, but all agog with the coming sport, until more than four hundred of the working men in Medicine were collected in the hall of the Lowell Institute, to hear the last effusion of the Professor, Poet and Wit, who rules over the morning meal, sparkles after dinner with toast and epigram, and takes his tea (without toast) at the Mutual Admiration Society, of which he is a prominent member.

It so happens, however, that the Professor, though clever, is a curious little man. He has a good opinion of himself. It's the only fixed principle he has got; for, in all other things, he glories in his free thinking. He must have a sensation, and has no idea of going over the road worn in many ruts by the slow coaches who preceded him. No, indeed; there must be some startling proposition to ruffle the surface of this complacent au-

dience, and nobody can get up the steam quicker than our orator. If you want a new poem, he will give it to you in a minute, (sometimes a right good one). Would you like a new religion? You can soon be made a member of "*The great American Church.*" Jerusalem and Rome are trampled into dust in the triumphant march of the "three-hilled city." Here are the geological "tables of stone," to be read by the wiseacres of Cambridge, for Moses and his law hardly suit the times. What though Paul the Apostle lies buried with the fathers, don't Parker and the Professor reign in his stead?

Well, to make a long story short, the Professor's imagination happened to take the wrong turn, and our poor friends who had been so long saving their little earnings for this eventful occasion, were the sufferers; for, to their unutterable astonishment, the leading man in their medical school—their selected orator—had turned king's evidence, and was prepared to show to them and the world the pernicious consequences of their art. They calkilated upon a soothing, agreeable cataplasm, applied with the gentlest hand, but found themselves cantharadized and croton-oiled at a fearful rate.

"Hasn't a man the right to think what he pleases and say what he thinks" in this glorious land of free love, free religion, and free thinking? "Isn't this the very aim and end of our institutions?" Dr. Holmes has taught you, gentlemen, what you know of physic. He has spent your money and signed your sheep-skin. Who has a better right than he to speak his mind freely?

You need not pass a resolution "that this Society disclaims all responsibility for the sentiments contained in this address." The Autocrat is still in demand; the seventeenth edition of the poems is in press, and everybody buys the Professor at the Breakfast-Table, although the M.D. to the author's name is carefully withdrawn from the title-page. Sit still, my friends, and don't make an unnecessary to-do. If he "should strain the truth for the sake of an epigram or an antithesis," can't a man lie a little, rather than spoil a joke?

"My friends and brothers in art," you seem to be satisfied

that you are practising a useful calling, which cures some, palliates much, prevents a good deal, and comforts all who require its aid. We told you so when at college, but now it is well to open your eyes to the truth. You are just helping those poor devils along a little faster, by stuffing down their throats a number of noxious drugs. "If the whole Materia Medica, as now used, could be thrown into the bottom of the sea, it would be all the better for mankind, and all the worse for the fishes."

We can make an exception of opium, "which the Creator seems to prescribe, as we often see the scarlet poppy growing in the corn-fields;" but all the other receipts of Omniscience must be condemned. The purple fox-glove, the many-tinted veratrum, the lilac stramonium, they are all "*noxious;*" but a little opium—it helps the imagination.

Wine must be preserved. [You don't catch as knowing a fellow emptying his demijohns into Charles River.] "Wine is food;" and "when a circle of clever fellows meet together to dine and have a good time of it," wine is "the great equalizer and fraternizer which works up the radiators to their maximum radiation, and the absorbents to their maximum receptivity."

Don't throw away "the specifics." [You need not look self-satisfied, my good man, you didn't discover one of them. The Jesuits and the quacks and other people told us all about them, and we know how they should be used without your advice.] But with wine comes the gout, and besides, I might get the chills and fever whilst peddling my tin cups about the country, (cheap article at fifty dollars apiece, and with more sodder in them than I intend *you* shall have to-day).

Chloroform is worth keeping, or, in more elegant phrase, "the miracle of anæsthesia"—as much a miracle as many that old fogies, strapped down by threadbare doctrines and antiquated creeds, think so much of. Away with all the rest.

[There is no mistake but the Professor did give his friends and brothers in art a handsome drubbing.] Don't you know "that every noxious agent, including medicines, which would hurt a well man, will hurt a sick one?" They did *not* know it; one pig-headed old grumbler, with a wide-brimmed white hat and

a hickory cane, had thought that disease and health are opposites, and what's a well man's poison *might* be a sick one's meat. Pshaw, says the Professor of Anatomy in Harvard, the best system of practice that I know, is the one "Nature taught to the first mother, when she saw her first-born child putting some ugly pebble or lurid berry into its mouth; I know not in what language it was spoken, but I know that in English it would sound thus: *Spit it out.*"

[At this last shot, old Broadbrim gave an awful growl, and I know not in what language it was spoken, but I *think* that in English it would have sounded thus: PUT HIM OUT.]

Mixing mirth and sentiment with "practical hints," the orator treated his audience to a few clinical remarks, worthy of Graves and Bennet, which ought to be preserved without mutilation:

"One practical hint may not be out of place here. It seems to be sometimes forgotten by those who must know the fact, that the tongue is very different, anatomically and physiologically, from the stomach. Its condition does not, in the least, imply that of the stomach, which is a very different structure, covered with a different kind of epithelium, and furnished with entirely different secretions. A silversmith will, for a dollar, make a small *hoe*, of solid silver, which will last for centuries, and will give a patient more comfort, used for the removal of the accumulated epithelium and fungous growths which constitute the 'fur,' than many a prescription with a split-footed ℞ before it, addressed to the parts out of reach."

[There were many quiet, but attentive auditors of this pronunciamento of the funny doctor, who were fresh from the fight with gastritis, dyspepsia and typhoid fever. They would not appreciate the Professor's anatomy, physiology, or beside knowledge.]

No, sir. Scrape their tongues. Massasoit had typhoid fever, and Governor Winslow scraped his tongue. The result was, that the grateful Indian, overwhelmed with the success of the practice, revealed a dark and murderous plot, and so preserved you all to this day. Yes, sir, it "may save the Union, some of

these times, if a Presidential candidate should be taken sick and his tongue wanted cleaning."

[Suppose you get in practice on those foul-mouthed fanatics and blasphemers of God and the Bible, who enjoy "the large license of speech in the thinking centre of this continent, the brain of the new world."]

Here is another spicy paragraph, which will amuse our readers "down in the provinces." They will be surprised if the graduates of Harvard, with the Professor to teach them clinical medicine, should be guilty of such blunders. Give it to them well: hilarious Blue-beard, tickle 'em to death:

"Again, see how the 'bilious' theory works in every-day life, here and now, illustrated by a case from actual life. A youthful practitioner, whose last molars have not been a great while cut, meets an experienced and noted physician in consultation. This is the case: A slender, lymphatic young woman is suckling two lusty twins, the intervals of suction being occupied on her part with palpitations, headaches, giddiness, throbbing in the head, and various nervous symptoms, her checks meantime getting bloodless, and her strength running away in company with her milk. The old experienced physician, seeing the yellowish waxy look which is common in anæmic patients, considers it a 'bilious' case, and is for giving a rousing emetic. Of course, he has to be wheedled out of this; a recipe is written for beef-steaks and porter, the twins are ignominiously expelled from the anæmic bosom, and forced to take prematurely to the bottle, and this prolific mother is saved for future usefulness in the line of maternity."

Suppose, however, that all these noxious agents were good for the diseases for which they are recommended, still I would say, Throw physic to the dogs. What, do you want to cure that wretch "with tabes mesenterica, hydrocephalus, and other similar maladies—*the race would be ruined, if art could ever learn to preserve the individuals subject to them?*"

[Leave them in the wilderness, says the savage, or the lonely cave. LET THEM DIE, responds the sentimentalist, poet, sage and physician, who greases the axle round which rolls the hub of the solar system.]

Going about the country, continues the excited humanitarian, trying to cure poor women of their pains and aches. "*They ought to have headaches and backaches and stomach-aches; they are not well if they do not have them.*"

[A trembling culprit shrinking under the lash—for he had been up all night with a sweet woman, mother of children, gentle of heart, who had one of the forbidden pains,—exclaimed: Most learned orator, did you *ever* try to ease the pangs of sickness, to smooth the couch of suffering?

Bless me, was the reply, wasn't I once the physician to the Broad-street District of the Boston Dispensary? "and if anybody got well under my care, it must have been in virtue of the rough-and-tumble constitution which emerges from the struggle for life in the street-gutters, rather than by the aid of my prescriptions."]

'Tis a pity that we have no room for more extracts from the Annual Address, and must imagine how the four hundred enjoyed their dinner after this traitorous onslaught from one who, honored and trusted, sat in the highest temple of Medicine and tried to deface its fair proportions. But, if the friends and brothers couldn't laugh, all the homœo-quacks, herb-doctors and eclectics enjoyed the joke hugely; the penny-a-liners quote the Professor in every newspaper, and advertisements introduce their vegetable remedies under the patronage of his high authority.

[Would some of our friends please send us a copy of the Professor's letter resigning his chair in the Medical School of Harvard? It should be placed upon record as a noble sacrifice of self on the altar of principle. He can never sign his name to the diploma of another "Vir, ingenio bono, ac scientia utili præditus, moribus que probis ornatus, post tempus usitatem medicinæ studio et praxi impensum." No—no, rather, at the behests of a higher law, see him and his gallant followers, as they riddle the accursed dens of Metcalfe or Chapman, and in glorious emulation of ancestral deeds, sending gallipots and pill-boxes to the bottom of Boston Harbor.] *Maryland and Virginia Med. Journal.*

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